

Tilburg University

Magnitude Effect in Intertemporal Allocation Tasks

Sun, Chen; Potters, Jan

Publication date:
2016

[Link to publication in Tilburg University Research Portal](#)

Citation for published version (APA):
Sun, C., & Potters, J. (2016). *Magnitude Effect in Intertemporal Allocation Tasks*.

General rights

Copyright and moral rights for the publications made accessible in the public portal are retained by the authors and/or other copyright owners and it is a condition of accessing publications that users recognise and abide by the legal requirements associated with these rights.

- Users may download and print one copy of any publication from the public portal for the purpose of private study or research.
- You may not further distribute the material or use it for any profit-making activity or commercial gain
- You may freely distribute the URL identifying the publication in the public portal

Take down policy

If you believe that this document breaches copyright please contact us providing details, and we will remove access to the work immediately and investigate your claim.

Magnitude Effect in Intertemporal Allocation Tasks

Chen Sun and Jan Potters*

October 2016

Abstract:

We investigate how the intertemporal allocation of monetary rewards is influenced by the size of the total budget, with a particular interest in the channels of influence. We find a significant magnitude effect: the budget share allocated to the later date increases with the size of the budget. This effect does not depend on whether the sooner reward is paid in the present or in the future, implying that the factors which drive the present bias cannot account for the magnitude effect. At the aggregate level as well as at the individual level, we find magnitude effects both on the discount rate and on intertemporal substitutability (i.e. utility curvature). The latter effect is consistent with theories in which the degree of asset integration is increasing in the stake.

Keywords:

time preference, magnitude effect, Convex Time Budget method

JEL-code: C91, D12, D81

* Sun: Department of Economics and CentER, Tilburg University (e-mail: c.sun@uvt.nl); Potters: Department of Economics and CentER, Tilburg University (e-mail: j.j.m.potters@uvt.nl). We are grateful for the helpful discussions with Arthur van Soest, and the insightful comments of Stephen Cheung, Mark Dean, Sebastian Ebert, Glenn Harrison, Gijs van de Kuilen, Guy Mayraz, Bin Miao, Wieland Müller, Karl Schlag, Yuehui Wang, Yuxin Yao, Songfa Zhong, and participants at the Tilburg mEETing, the Xiamen Workshop on Experimental Economics 2014, the ENTER Jamboree, the Tilburg Economics Workshop, the MBEES 2015, the 30th Annual Congress of the EEA, the ESA European meeting 2015 and the IMEBESS 2016. We thank the Netherlands Organization of Scientific Research (NWO) for financial support.

The prediction of the standard consumption-saving model, that people always discount an income at the market interest rate, has been found to be inconsistent with empirical results.¹ One important anomaly, dating back to Thaler (1981), is the magnitude effect: people appear less patient when choosing among smaller rewards than when choosing among larger rewards. A deeper understanding of this anomaly will help to lay a more solid foundation for the research of intertemporal choice and related applications.

In this paper, we investigate whether the magnitude effect on time preferences can be observed in intertemporal allocation tasks, and if so, whether the magnitudes impact intertemporal preferences through the present bias, the discount rate or the atemporal utility function.

Several experiments on time preferences have reported a magnitude effect.² Though most early studies are based on hypothetical decisions, there are also some real-stake experiments that found a magnitude effect (Holcomb and Nelson, 1992; Kirby, 1997; Kirby, Petry and Bickel, 1999; Andersen, Harrison, Lau, and Rutström, 2013; Halevy, 2015). In this literature, little efforts are made to explore the channels of the magnitude effect. This is mainly because most studies employed a *single-reward task*, in which a subject can only get one reward, either on a sooner date or on a later date.³ With a single-reward task, one cannot disentangle different channels and can only attribute all effects to one aggregate measure, the monetary discount rate.

We are interested in the following question: Is the magnitude effect mainly driven by the factors which drive the present bias, does the magnitude affect choices

¹ To be more precise, people discount at the market rate unless the borrowing constraint is binding.

² Frederick et al. (2002, Section 4.2.2) summarized the early literature on the magnitude effect of time preferences. Andersen et al. (2011) also reviewed the more recent literature.

³ Tasks of this kind include the binary choice task (including those in a Multiple Price List), the present/delayed equivalence task, and the indifferent date task. See Frederick et al. (2002, Section 6.2.2) for a review of the experimental studies on measuring discount rates.

through the long-run discount rate, or does it affect choices through intertemporal substitutability (utility curvature)? To disentangle these channels is interesting for at least two reasons. First, the knowledge about how the stake affects intertemporal choices in different ways is important for establishing deeper and better-founded descriptive theories of intertemporal decision making. Second, omitting a channel of the magnitude effect in an empirical study or in policy making may lead to misspecified models and biased estimates and predictions.

We employ the Convex Time Budget (CTB) method introduced by Andreoni and Sprenger (2012). It allows subjects to form a portfolio of a sooner reward and a later reward given a budget constraint. The possibility for subjects to make interior choices (and not only corner choices as in binary choice tasks) enables the researcher to simultaneously identify the discount rate and the intertemporal substitutability.

The design of our experiment has three main features. First, all subjects receive equal amounts of participation fees on the sooner date and on the later date regardless of their choices, and the payment conditions are constant across time. Thus the transaction costs and the trustworthiness of the payments are equalized across periods, and these confounding factors are controlled for. Second, we implement two treatments. In one treatment subjects allocate between today and four weeks later, while in the other treatment subjects allocate between four weeks later and eight weeks later. This allows us to assess whether the magnitude effect is driven by the same factors that drive the present bias. Finally, by assuming a simple yet popular model, the CTB method allows us to identify the discount rate and the atemporal utility function simultaneously. As a result, we are able to disentangle the channels of the magnitude effect.

We find evidence of the magnitude effect, irrespective of whether or not a front-end delay is present, suggesting that the factor which drives the present bias cannot fully explain the magnitude effect. The magnitude effect is decreasing in the

magnitude. At the aggregate level as well as at the individual level, we find magnitude effects both on the discount rate and on intertemporal substitutability. Both channels have considerable impacts on predicted choices. We find that the latter effect is not the same as the magnitude effect on risk attitudes, and hence it might be problematic to correct for the curvature of utility functions by risk attitudes.

The remaining part of this paper is structured as follows: We introduce our experimental design in Section I. In Section II we formulate our hypotheses. In Section III, we investigate non-parametrically the magnitude effect and its relation with the present bias. We explore the channels by parametric estimation both at the aggregate level and at the individual level in Section IV. In Section V, we discuss the interpretations of our findings. We draw conclusions in Section VI.

I. Experimental Design

A. The Convex Time Budget Method, Parameters and Implementation

The foundation of our experimental design is the Convex Time Budget method introduced by Andreoni and Sprenger (2012). The method consists of a set of intertemporal allocation tasks: in each *decision* subjects are asked to allocate N tokens to two dates, t days from today and $(t + \tau)$ days from today. Each token allocated to t is worth P_t euro, while each token allocated to $(t + \tau)$ is worth $P_{t+\tau}$ euro. Suppose a subject allocates n_t tokens to the sooner date and $n_{t+\tau}$ to the later date, the amount of the sooner reward will be $z_t = P_t \cdot n_t$ euro and the amount of the later reward will be $z_{t+\tau} = P_{t+\tau} \cdot n_{t+\tau}$ euro.

Subjects are subject to the budget constraint, $n_t + n_{t+\tau} \leq N$, and the boundary constraints, $0 \leq n_t, n_{t+\tau} \leq N$. They are told that they can allocate any number of tokens they like to one of the two dates. Examples of both corner choices and interior choices are given to remove any hesitation in making either type of choices.

Decisions with the same total budget, N , are grouped in one *decision form*, which is displayed on one page. There are seven decisions in one decision form. The return to each token allocated to the later date is fixed as $P_{t+\tau} = \text{€}0.20$, while the return to each token allocated to the sooner date is varied and takes the values $P_t = \text{€}0.20, \text{€}0.19, \text{€}0.18, \text{€}0.17, \text{€}0.16, \text{€}0.15$, and $\text{€}0.14$. Hence, those decisions imply seven gross interest rates, $R = 1, 1.05, 1.11, 1.18, 1.25, 1.33$, and 1.43 , respectively, over a period of τ days. The constraints can be rewritten as

$$R \cdot z_t + z_{t+\tau} \leq m$$

$$z_t, z_{t+\tau} \geq 0$$

where m is the total budget and $m = P_{t+\tau} \cdot N$.

We implement the CTB method by a zTree program (Fischbacher 2007). Figure 1 shows the interface of a typical decision form. Each decision takes a row. Decisions can be made by scrolling the bars. Once an adjustment is made for one decision, the amounts of the sooner reward and of the later reward in that decision are automatically calculated and displayed.

To avoid any possible effects of initial values, the amounts of rewards are initially blank. Decisions cannot be submitted until all the scrollbars have been adjusted at least once.

B. Procedures

There are two parts in our experiment. Part I consists of five decision forms, with $N = 100, 200, 300, 400$, and 800 . The order is randomly drawn for each subject.⁴ Subjects can move to a specific decision form by clicking the button with the corresponding number. One can go to any decision form at any time, regardless of whether the current decision form is completed. Decisions are automatically stored

⁴ We find no evidence of order effect at the aggregate level.

when one switches to another decision form. This makes comparisons across magnitudes very easy to the subjects, in case they would want to make such comparisons. Decisions can only be submitted when all the 35 decisions in the five decision forms are completed.

We randomly assign subjects to one of two treatment groups. In the Present Group, the sooner date is today while the later date is four weeks from today, i.e. $t = 0$ and $\tau = 28$. In the Delayed Group, the sooner date is four weeks from today while the later date is eight weeks from today, i.e. $t = 28$ and $\tau = 28$. Comparing the two groups enables us to check if there exists a present bias on average, and more importantly, if there exists a magnitude effect when no rewards are available in the present.

Form 1: Decisions 1 - 7

September 2014	October 2014	November 2014	December 2014
1 2 3 4 5 6 7	1 2 3 4 5	1 2	1 2 3 4 5 6 7
8 9 10 11 12 13 14	6 7 8 9 10 11 12	3 4 5 6 7 8 9	8 9 10 11 12 13 14
15 16 17 18 19 20 21	13 14 15 16 17 18 19	10 11 12 13 14 15 16	15 16 17 18 19 20 21
22 23 24 25 26 27 28	20 21 22 23 24 25 26	17 18 19 20 21 22 23	22 23 24 25 26 27 28
29 30	27 28 29 30 31	24 25 26 27 28 29 30	29 30 31

You are required to allocate 300 tokens between the two dates for each decision:

	Payment A: 20 Oct 4 WEEKS from Today	and	Payment B: 17 Nov 8 WEEKS from Today	20 Oct	17 Nov
1	265 tokens at € 0.20 each on 20 Oct	and	35 tokens at € 0.20 each on 17 Nov	€ 53.00	€ 7.00
2	tokens at € 0.19 each on 20 Oct	and	tokens at € 0.20 each on 17 Nov	€	€
3	tokens at € 0.18 each on 20 Oct	and	tokens at € 0.20 each on 17 Nov	€	€
4	tokens at € 0.17 each on 20 Oct	and	tokens at € 0.20 each on 17 Nov	€	€
5	tokens at € 0.16 each on 20 Oct	and	tokens at € 0.20 each on 17 Nov	€	€
6	tokens at € 0.15 each on 20 Oct	and	tokens at € 0.20 each on 17 Nov	€	€
7	tokens at € 0.14 each on 20 Oct	and	tokens at € 0.20 each on 17 Nov	€	€

Switch to Form

12345

Submit Decisions
<--Clicking this button will submit ALL your decisions in every form

Figure 1. Interface of a typical decision form in Part I

Part II is composed of an extended CTB decision form with seven decisions. Subjects are asked to allocate 400 tokens to three dates, today, four weeks from today and eight weeks from today. One additional restriction is imposed, depending on which group one is in. A subject in the Present Group can allocate either 0 or 200 tokens to eight weeks from today; she cannot choose other numbers. But she is still free to allocate any number of tokens between today and four weeks from today. Similarly, a subject in the Delayed Group can allocate either 200 or 400 tokens to today. She is still free to allocate any number of tokens (if there remains some) between four weeks from today and eight weeks from today. The restrictions and the returns to one token allocated are shown in Table 1.

Table 1. Restrictions on the number of tokens and returns to one token allocated to a specific date in Part II

Group		Today	Four weeks from today	Eight weeks from today
Present	Returns to one token	€0.20, €0.19, €0.18, €0.17, €0.16, €0.15, €0.14	€0.20	€0.26
	Restriction on the number of tokens	No restriction	No restriction	0 or 200
Delayed	Returns to one token	€0.08	€0.20, €0.19, €0.18, €0.17, €0.16, €0.15, €0.14	€0.20
	Restriction on the number of tokens	200 or 400	No restriction	No restriction

The additional date (eight weeks from today for the Present Group or today for the Delayed Group) is accompanied with a very high return for the Present Group and a very low return for the Delayed Group, so that subjects are induced to allocate 200 tokens to this additional date.⁵ If they do so, the remaining task is equivalent to the one with a total budget of 200 tokens in Part I. This characteristic makes the two decision forms comparable.

⁵ In fact, only nine out of 203 subjects selected a different number than 200 to the additional date, which involved 41 (2.9%) out of 1415 decisions.

The purpose of Part II is to test the time separability of intertemporal preferences. One alternative hypothesis is that a subject in the Delayed Group may allocate less to the sooner date if she has allocated a large amount of money to an even sooner date, since the desire for extra consumption has already been partly satisfied. A similar hypothesis applies to the Present Group: a subject in the Present Group may allocate less to the later date if she has already allocated a large amount of money to an even later date, since the guilt for not saving has been partly released. If preferences are time non-separable, the use of a model with a time separable preference is more likely to be problematic. Thus we want to test the hypothesis of time separability before we perform parametric estimation with a time-separable model.

We do not directly give a fixed reward on the additional date. This is because a fixed reward might be mentally isolated from the allocation task due to narrow bracketing, and hence the test of time separability in the allocation task may be invalid.

At the end of the experiment, subjects were asked to finish a questionnaire. As in previous studies with the CTB method, we asked about subjects' typical expenditures in one week. The average response was €55.22 per week or €7.89 per day.

C. Experimental Payments

The payments are composed of two parts. First, all subjects receive a €5 *participation fee* on each of the two dates scheduled in Part I. Second, each subject has a 10% chance to receive *earnings from decisions*. Before the experiment starts, each subject is randomly given a lottery number, ranging from 0 to 9. After all subjects in a session finish the questionnaire, the experimenter invites one of the subjects to draw a ten-sided die in front of all subjects in the session. Subjects who

have a lottery number that equals the die roll get the earnings from decisions. One decision is randomly selected from the 42 decisions in the two parts as the *decision that counts*. If the decision that counts is from Part I, the allocation in that decision will be realized as the earnings from decisions. If the decision that counts is from Part II, the allocation will be realized and the subject will also receive a €5 participation fee on the additional date in Part II; hence a subject will receive three participation fees if a decision in Part II is realized. All the rules above were articulated in the instruction, and the instructions were always read aloud before either part of the experiment.

The earnings were paid by bank transfer to subjects' checking accounts. We made orders of transfers soon after the experiment, and sent reminder emails with information about the incoming amounts on the experimental day and on all the payment dates. Given the reliability of the banking service, subjects can expect to receive all delayed payments exactly on the appropriate payment dates, while some of the present payments might be received one day after the experimental day due to the inter-bank processing.

We believe the payment tool we used was as good as cash in terms of liquidity. Checking accounts are used in private transactions such as paying for rents. Checking accounts are also linked to debit cards. In the Netherlands, debit cards are widely used for daily transactions in almost all kinds of stores including supermarkets, university restaurants and bookstores without any transaction fees. We held a survey about subjects' use of debit cards in the questionnaire. The responses show that bank transfers give high liquidity to the rewards, so that no isolation effect should be expected due to the payment method.⁶

⁶ 84.7% of the subjects pay at least 50% of their expenditure in general by debit card, while 91.1% pay at least 30% of their expenditure in general by debit card. Among those who pay less than 30% of their expenditure in general by debit card, 61.1% pay at least 30% of their expenditure in university restaurants or in supermarkets by debit card. Among the remaining seven subjects, four withdraw cash at least 3 times per month.

D. Transaction Costs and Credibility of Payments

For our experiment, it is extremely important to equalize the transaction costs and the trustworthiness of the payments across periods, because a difference in the transaction costs over the two periods can be a confounding factor of the magnitude effect.

Several facilities were employed in order to equalize the transaction costs across periods and to increase the credibility of the payments. The transaction costs include the costs to collect rewards, to confirm that the rewards have been received with correct amounts, and to remember the earnings so that they can be consumed on the expected dates.

First, we sent reminder emails with information about the incoming amounts on the experimental day and on all the payment dates. Subjects knew this from the instruction, so they did not need to worry about forgetting the earnings on the payment dates, a situation in which the expected marginal utility of the delayed rewards might be lowered.

Second, as Andreoni and Sprenger (2012) did, we delivered our business card and told the subjects to contact us immediately in case they would not receive a payment on time. It increased the credibility of payments and meanwhile served as a reminder of the payments.

Third, we asked subjects to fill in a payment reminder card with the amounts of their rewards on the corresponding dates just after their earnings were displayed. This served as a second reminder in case they forget to check emails.

In sum, the characteristics that one will receive a participation fee on each payment date and that all payments will be received by bank transfer help equalize the transaction costs of receiving payments on all dates. At the same time, the business cards, the payments reminder cards and the reminder emails reduced the risk of forgetting the rewards. The business cards also lowered the perceived default

risks. Even though the risk might still be perceived by some subjects, it should be equal across periods since the payment tools and all auxiliary facilities were the same.

E. Sample

Our experiment was conducted at the CentERlab, Tilburg University in September of 2014.⁷ 203 students of the university participated in one of the 11 sessions, 94 in the Present Group and 109 in the Delayed Group. Each subject made 42 decisions. One session took one hour and ten minutes on average. 22 subjects got the earnings from decisions, which averaged €69.16. The overall average earning was €17.49.

II. Hypotheses

The focus of this paper is on whether there is a magnitude effect on time preference, i.e. if people make intertemporal choices differently when the stakes vary. However, the definition of the magnitude effect still needs to be clarified.

In single-reward tasks, subjects reveal an indifference relation between a smaller sooner reward and a larger later reward, i.e. $(m_t, t) \sim (m_{t+\tau}, t + \tau)$. Accordingly, the magnitude effect on time preference is defined on the monetary discount rate,

$d_m \equiv \left(\frac{m_{t+\tau}}{m_t} \right)^{\frac{1}{\tau}} - 1$. A common result of such studies is that the monetary discount rate is decreasing in the stake, i.e. the monetary discount factor is increasing in the stake, which can be called as a positive magnitude effect on the monetary discount rate.

⁷ The payment dates were in September, October and November. The fall semester in Tilburg University started from the end of August and ended in early December. Hence the payment dates were earlier than the final exam weeks and the Christmas vacation, which keeps our experiment from their probably large impacts on the subjects' demand of money.

In our intertemporal allocation task, the monetary discount rate cannot be defined, unless a subject always puts all the tokens onto one of the two dates. Therefore, the magnitude effect needs to be redefined. A natural replacement is the budget share. If the budget share allocated onto the later reward ($\frac{n_{t+\tau}}{N}$, or equivalently $\frac{z_{t+\tau}}{m}$) is increasing in the size of the total budget (m), we call this a positive *magnitude effect on budget share*. Thus, our first hypothesis is

Hypothesis 1 (magnitude effect on budget share): $\frac{z_{t+\tau}}{m}$ is increasing in m .

This hypothesis can be tested without assuming a specific model.

We are also interested in the relationship between the present bias and the magnitude effect. Benhabib et al. (2010) suggest that a fixed cost of delaying rewards can account for the present bias and the magnitude effect simultaneously, since the fixed cost induces the decision weight of future rewards to change disproportionately with delay and with the size of rewards. We would like to know if this cost is incurred only when a present reward is delayed or if it also applies to delaying a future reward. In a broader sense, we test whether the factors that drive the present bias can account for the magnitude effect. If so, we should observe a magnitude effect in the Present Group, but not in the Delayed Group. Thus, we establish our second hypothesis.

Hypothesis 2 (no present reward, no magnitude effect): $\frac{z_{t+\tau}}{m}$ does not change with m in the Delayed Group.

Conditional on finding a positive magnitude effect, we wish to explore the channels of the magnitude effect. Given the evidence of time separability, we will estimate the parameters of preferences, with the assumption that subjects maximize a time separable utility function with CRRA atemporal utility functions and quasi-hyperbolic discounting, i.e. subjects maximize

$$(1) \quad U(z_t, z_{t+\tau}) = \frac{1}{\alpha} \delta^t (z_t + \omega)^\alpha + \beta \delta^{t+\tau} \frac{1}{\alpha} (z_{t+\tau} + \omega)^\alpha,$$

where β is the present bias parameter, δ is the daily discount factor, α is the exponent parameter. z_t and $z_{t+\tau}$ are the sooner reward and the later reward, respectively. ω is the background consumption mentally integrated with the experimental reward when the decision is made.

When the CRRA utility function is assumed, the elasticity of intertemporal substitution in consumption, $e_c \equiv -\frac{\ln\left(\frac{c_{t+\tau}}{c_t}\right)}{\ln\left(\frac{u'(c_{t+\tau})}{u'(c_t)}\right)}$, is equal to $\frac{1}{1-\alpha}$ (c_t and $c_{t+\tau}$ are the consumption on the sooner date and on the later date, respectively.). Thus the exponent parameter, α , is a positive transformation of e_c . If $\alpha \rightarrow 1$, the atemporal utility function becomes linear, and the elasticity goes to infinity. In that case, subjects just go for the largest present value, and hence rewards are perfectly substitutable between dates. In case $\alpha \rightarrow -\infty$, the atemporal utility function is Leontief, and the elasticity goes to zero. In that case, subjects always divide the total budget into two equal amounts. In general, the larger the value of α , the more substitutable the subject considers the two rewards to be. Therefore, α is a measure of intertemporal substitutability.

It brings several advantages to assume such a model. First, the parameters in this model have important economic meanings. The discount factor determines the average choice across interest rates, and measures the patience of the subject; if a subject is more patient, she will allocate more tokens to the later date for all interest rates. The intertemporal substitutability of consumption at different points in time relates to the dispersion of the choices across interest rates, since it measures the sensitivity of the subject to the interest rate. These behavioral measures are hard to estimate without assuming a model. Due to the boundary constraint, choices are censored at the corners if the preference parameters are extreme. As a result, directly measuring the average choice (as a measure of δ) and the dispersion of choices (as a measure of α) leads to biases. In contrast, the model we assume is

tractable and easy to estimate. Moreover, the model is widely used in both theoretical and empirical applications.⁸

Given the model above, we test the following two hypotheses.

Hypothesis 3 (magnitude effect on discount factor): δ is increasing in m .

Hypothesis 4 (magnitude effect on intertemporal substitutability): α is increasing in m .

⁸ To address the concern about misspecification, in Appendix B, we check the robustness of our results by estimating a model with the Hyperbolic Absolute Risk Aversion (HARA) utility function and quasi-hyperbolic discounting. The HARA utility function is more flexible in the sense that it allows the atemporal utility function to be Increasing Relative Risk Aversion, Constant Relative Risk Aversion or Decreasing Relative Risk Aversion. This kind of flexibility is especially important when the magnitude is varied in the experiment. The results are the same. Also see Appendix D for a further discussion about whether a more general utility function can rationalize our results.

III. Overall Effects

A. Magnitude Effect on Budget Share

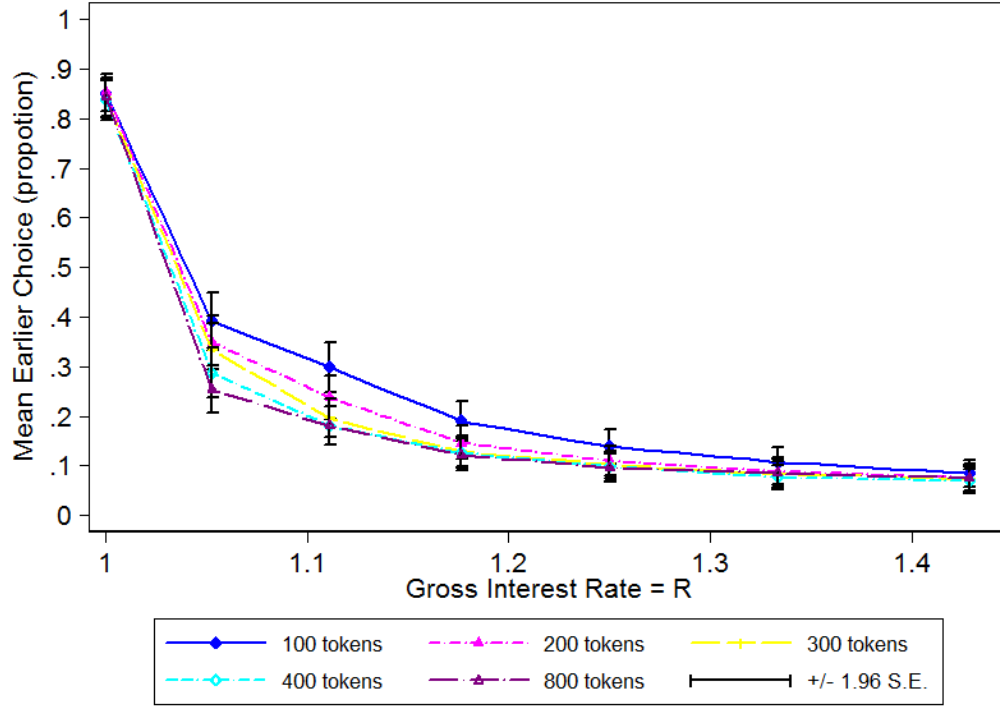


Figure 2. Mean Budget Share on the Sooner Date in Part I

In Figure 2 we plot the mean budget share allocated to the sooner date against the gross interest rate, R , of each CTB decision in Part I.⁹ We plot separate points for the five magnitudes ($m = \text{€}20, \text{€}40, \text{€}60, \text{€}80, \text{€}160$). The budget share allocated to the sooner date declines with the magnitude.

The difference seems to be larger when the interest rate is smaller but still positive. This is mainly due to censoring. When the interest rate is zero ($R = 1$) or highest ($R = 1.43$), most choices are at the corners for both smaller and larger magnitudes.

⁹ In our data, 72% of the choices are at corners, and 38% of our subjects only make corner choices. This is very close to the 70% and 37%, respectively, in Andreoni and Sprenger (2012). The relationships between the budget shares and the interest rates are also similar.

To judge if there is a significant magnitude effect, we first perform signed-rank tests on the mean difference in allocations for each pair of consecutive magnitudes and each interest rate. As can be seen in Table 2, for 5 out of 7 interest rates the mean differences are significant between the magnitude of €20 and €40. For larger magnitudes, the mean differences are significant for only one or two of the seven interest rates.

Table 2. Univariate mean difference tests between magnitudes

	<i>R</i>						
	1	1.0526	1.1111	1.1765	1.25	1.3333	1.4286
€20 ~ €40	-0.0014	0.0417**	0.0613***	0.0438**	0.0286**	0.0186	0.0096*
€40 ~ €60	0.0132	0.0157	0.0421	0.0179**	0.0078	0.0053	0.0015
€60 ~ €80	0.0027	0.0463***	0.0142	0.0024	0.0041	0.0067	0.0042
€80 ~ €160	-0.0066	0.0335**	0.0008	0.0039*	0.0020*	-0.0076	-0.0058

Notes: Mean differences in the budget shares between two consecutive magnitudes given a gross interest rate, *R*, and the results of Wilcoxon signed-rank tests on them. 203 observations for each magnitude and each interest rate. ***, ** and * indicate significance at the 1 percent level, 5 percent level, and 10 percent level, respectively.

We then perform Hotelling's T-squared tests on the mean differences in budget shares between magnitudes, taking seven choices with the same interest rate as a vector (see Table 3).¹⁰ The null hypothesis is that the means of choices are the same across magnitudes, taking into account the correlation within subject. This class of tests makes sense because individual heterogeneity may have made different subjects reveal magnitude effects on tasks with different interest rates (e.g. Subject 1 on Interest Rate 1 while Subject 2 on Interest Rate 2), so that the magnitude effects on all choices would be jointly significant, but the effect on choices with any single interest rate might not be significant. The results show that the magnitude effect is significant between the magnitudes of €20 and €40 and between any two non-adjacent magnitudes. These results support Hypothesis 1, which states that a larger

¹⁰ Hotelling's T-squared test is asymptotically nonparametric, so it can be applied to a large sample in nonnormal cases. We also perform a multivariate signed-rank test (Oja and Randles, 2004) and the results are basically the same: the magnitude effects are significant between the magnitudes of €20 and €40 and between any two non-adjacent magnitudes at least at the 10% level.

share of the budget is allocated to the later date when the size of the budget increases.

11

The results also show that the differences are insignificant between adjacent magnitudes larger than €20. Since the allocation is monotonic in the magnitude and the differences are significant between non-adjacent magnitudes, the insignificance suggests that the magnitude effect is largest when comparing the smallest magnitudes (€20 and €40), and becomes smaller for larger magnitudes. The pattern is consistent with the fact that Andersen et al. (2013) only found a small magnitude effect when they elicited time preferences using very high stakes.¹²

Table 3. Multivariate mean difference tests between magnitudes

		€20 ~ €40	€40 ~ €60	€60 ~ €80	€80 ~ €160	€20 ~ €60	€40 ~ €80	€60 ~ €160
Total	F-statistic	3.3184***	1.5477	1.5761	1.7104	5.1162***	3.9336***	3.0328***
	p-value	0.0023	0.1533	0.1444	0.1084	0.0000	0.0005	0.0047
Present Group	F-statistic	2.4091**	1.2374	1.6696	1.3919	3.2252***	2.9725***	2.0495*
	p-value	0.0266	0.2913	0.1270	0.2190	0.0044	0.0076	0.0577
Delayed Group	F-statistic	2.4388**	1.3650	1.0096	1.3659	2.8009**	2.1048**	2.0595*
	p-value	0.0237	0.2282	0.4290	0.2278	0.0104	0.0495	0.0547

Notes: Hotelling's T-squared tests on the mean differences in the budget shares between two magnitudes for all gross interest rates. 203 sets of observations for each magnitude. The degrees of freedom of the F-statistics are (7,196) in total, (7, 87) in the Present Group and (7, 102) in the Delayed Group. ***, ** and * indicate significance at the 1 percent level, 5 percent level, and 10 percent level, respectively.

The results are robust against multiple hypotheses testing problem, since performing a Holm-Bonferroni correction on any family of four/three hypotheses does not change the significance at the 10 percent level.

B. Present Bias

Table 3 shows the results of the Hotelling's T-squared tests for the Present Group and for the Delayed Group separately. We find significant magnitude effects in both

¹¹ In Table 3 statistics are reported only for four pairs of non-adjacent magnitudes. The mean differences for the other three pairs of non-adjacent magnitudes (in the two groups separately and in total) are also significant: seven out of the nine differences are significant at the 1% level, while the other two at the 5% level.

¹² They compare magnitudes of 1,500 Danish kroner and 3,000 Danish kroner, which is roughly equivalent to €200 and €400, respectively.

groups. This implies that the presence of an immediate reward is not a necessary condition for the magnitude effect. In other words, the factor that drives the present bias is unlikely to be the driver of the magnitude effect. Thereby, we reject Hypothesis 2.

We plot separate graphs for the two groups in Figure 3. Subjects in the Delayed Group seem to be slightly more patient than those in the Present Group. However, when we perform the Hotelling's T-squared test on all the 35 decisions in Part I between groups, the null hypothesis that the two groups have the same mean responses is not rejected. The p-value is 0.2424 when the degree of freedom is (35, 167). Thus we find no evidence of present bias.¹³

The absence of a present bias is consistent with the results of Andreoni and Sprenger (2012). They considered that one possible reason might be that even the present rewards were paid by the checks sent to the students' mailbox, so no reward was truly immediate. Since we have a similar payment procedure, it is not strange that we have the same result. Another possible reason for not finding a present bias is that subjects only have a 10% chance to receive the earnings from decisions. As first suggested by Keren and Roelofsma (1995), the present bias may be less salient in case rewards are risky than in case rewards are certain.

Even though there might be a present bias which is not captured by the CTB method, our results still have two implications for the magnitude effects. First, since a magnitude effect is present when the present bias is absent, our results imply that the factor which drives the present bias cannot fully account for the magnitude effect. Second, if it is a mental cost of delaying rewards that drives the magnitude

¹³ The present bias here refers to non-stationarity of preferences according to the categorization of Halevy (2015). Our finding does not necessarily contradict the stylized fact that the discount rate is decreasing in the time distance between the sooner reward and the later reward, as in Benhabib et al. (2010). That stylized fact and stationarity can hold simultaneously if there is subadditivity in discounting (see Read, 2001).

effect (Benhabib and Bisin, 2005; Fudenberg and Levine, 2006), our results suggest that a similar mental cost is incurred when one postpones a future reward.

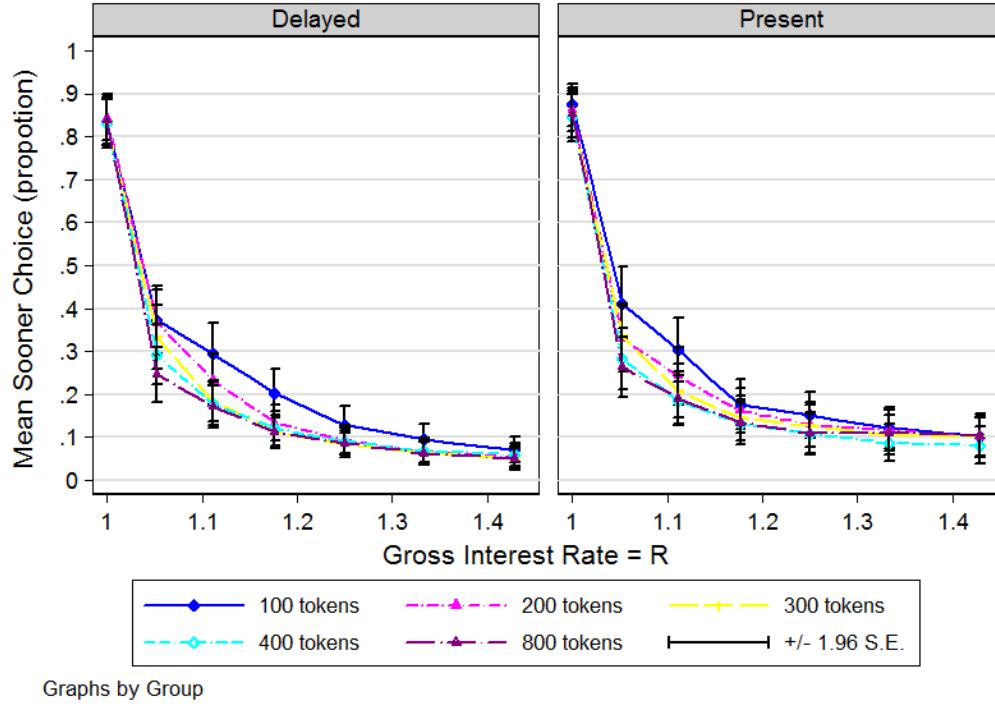


Figure 3. Mean budget shares in the Present Group and in the Delayed Group

C. Time Separability

The outcomes show that Part II is a valid test of time separability, since most subjects chose 200 tokens for the additional date in Part II. Only 39 out of 658 decisions from the Present Group and two out of 763 decisions from the Delayed Group were different from 200 tokens. Those involved eight subjects in the Present Group and one subject in the Delayed Group.

After removing those decisions, we compare the choices with the magnitude of €40 between Part I and Part II, separately for each group. Table 4 shows that the Hotelling's T-squared tests fail to reject the null hypothesis that responses to the

two parts have the same means. Those results support time separability, which will be used in the following section.

Table 4. Multivariate mean difference tests between Part I and Part II

Subsample	Present	Delayed	Total
F-statistic	1.5560	1.4192	1.0979
Degree of freedom	7, 79	7, 101	7, 187
p-value	0.1609	0.2058	0.3662

Notes: Hotelling's T-squared tests on the mean differences in the budget shares in the decisions with the magnitude of €40 between Part I and Part II. Subjects who chose a different number from 200 tokens for the additional date in Part II such that their choices were not comparable between the two parts have been removed from the sample. ***, ** and * indicate significance at the 1 percent level, 5 percent level, and 10 percent level, respectively.

IV. Channels

In order to disentangle the magnitude effect into two channels, we perform parametric estimations both at the aggregate level and at the individual level. We then test if the preference parameters change with the magnitude of the total budget.

A. Aggregate-level estimation

1. Estimation strategy

In our main specification, we assume a CRRA atemporal utility function as in equation (1). We set ω (background consumption) equal to the average response to the question about one's typical daily expenditure, €7.89, as Andreoni and Sprenger (2012) did in two of their specifications.¹⁴

Given the intertemporal utility function, solving the optimization problem yields the tangency condition

¹⁴ To fix the background consumption across subjects brings the advantage that all effects come from the variation in choices rather than also from the variation in the self-reported background consumptions, which may be noisy. We check the robustness by setting ω as individual background consumption, and average/individual background consumption combined with the participation fee (See Appendix A). The results are basically the same.

$$\frac{z_t + \omega}{z_{t+\tau} + \omega} = \begin{cases} (\beta \delta^\tau R)^{\frac{1}{\alpha-1}}, & \text{if } t = 0 \\ (\delta^\tau R)^{\frac{1}{\alpha-1}}, & \text{if } t > 0 \end{cases}.$$

Taking logs gives a linear equation

$$\ln \left(\frac{z_t + \omega}{z_{t+\tau} + \omega} \right) = \left(\frac{\ln \beta}{\alpha - 1} \right) \cdot 1_{t=0} + \left(\frac{\ln \delta^\tau}{\alpha - 1} \right) + \left(\frac{1}{\alpha - 1} \right) \cdot \ln R$$

where $1_{t=0}$ is the indicator for the Present Group.

The parameters to be estimated are the present bias parameter, β , the discount factor, δ , and the CRRA curvature parameter, α . The present bias parameter is identified by the differences in allocation between the Present Group and the Delayed Group. If there is a present bias, subjects in the Present Group will allocate more tokens to the sooner date than those in the Delayed Group. The discount factor is identified by one's average choice across different experimental interest rates. A more patient subject will allocate more tokens to the later date in all decisions. The curvature parameter is identified by the dispersion of one's choices across interest rates. Those who consider rewards highly substitutable over time are likely to make corner choices in all decisions, while those with lower elasticity of intertemporal substitution will make choices closer to equal splits.

Following the practice in previous studies (Andreoni and Sprenger, 2012; and Augenblick, Niederle and Sprenger, 2015), we assume a normally distributed error term additive to the log allocation ratio and take censoring into consideration, then we yield the two-limit Tobit model:

$$\begin{aligned} l_{i,j,k}^* &\equiv \ln \left(\frac{z_{t;i,j,k}^* + \omega}{z_{t+\tau;i,j,k}^* + \omega} \right) \\ &= \left(\frac{\ln \beta}{\alpha - 1} \right) \cdot 1_{t=0;i} + \left(\frac{\ln \delta^\tau}{\alpha - 1} \right) + \left(\frac{1}{\alpha - 1} \right) \ln R_j + \epsilon_{i,j,k}, \epsilon_{i,j,k} \sim N(0, \sigma_k) \end{aligned}$$

$$l_{i,j,k} = \begin{cases} \ln \frac{\omega}{m_k + \omega}, & \text{if } l_{i,j,k}^* \leq \ln \frac{\omega}{m_k + \omega} \\ l_{i,j,k}^*, & \text{if } \ln \frac{\omega}{m_k + \omega} < l_{i,j,k}^* < \ln \frac{\frac{m_k}{R_j} + \omega}{\omega} \\ \ln \frac{\frac{m_k}{R_j} + \omega}{\omega}, & \text{if } l_{i,j,k}^* \geq \ln \frac{\frac{m_k}{R_j} + \omega}{\omega} \end{cases}$$

where $i = 1, \dots, 203$ denotes Subject i , $j = 1, \dots, 7$ denotes Interest rate j , and $k = 1, \dots, 5$ denotes Magnitude k . The error term is allowed to vary across magnitudes since giving a larger number of tokens might induce a larger noise, which might be a competing explanation of a larger sensitivity to the interest rate.

The model is estimated by the quasi-maximum-likelihood method: when performing the estimation, the error term, ϵ , is assumed to be i.i.d., while in computing the standard errors, the error term is assumed to be independent across subjects, but might be correlated within-subject. Estimates of the parameters can be recovered and standard errors can be inferred by the delta method.

Since we are interested in the magnitude effect, we also perform the estimation with interaction terms of the parameters and the magnitude dummies. Thus tests can be performed on the differences between the parameters for different magnitudes.

In Appendix B, we assume another specification, in which the utility function is Hyperbolic Absolute Risk Aversion (HARA). In that specification the background consumption, ω , is also a parameter to be estimated. In this way, we address the concern that the average self-reported background consumption may not match the true background consumption integrated with the experimental rewards in decision making, or the Relative Risk Aversion of the utility function may not be constant (i.e. the CRRA utility function is misspecified). The results are basically the same.

2. Results

Table 5 reports the magnitude-invariant estimates and the magnitude-specific estimates of the parameters, respectively. A salient feature is that none of the estimates of β is significantly different from 1, implying no evidence of present bias, which is consistent with our finding in the model-free analysis. The annual discount rate for all magnitudes is 52.7%, which is in the range found by previous studies. The CRRA curvature parameters are always significantly smaller than 1, implying that the subjects on average consider the monetary rewards received on different dates imperfectly substitutable, which is also consistent with other studies (e.g. Andreoni and Sprenger, 2012; Andreoni, Kuhn and Sprenger, 2013; Cheung, 2015; and Augenblick, Niederle and Sprenger, 2015).

Most importantly, both the discount factor and the CRRA curvature are increasing in the magnitude. To judge if these magnitude effects are significant, Table 6 presents Wald tests over the differences of parameters between magnitudes.¹⁵ We find significant magnitude effects both on the discount factor, δ , and on the exponent parameter, α , which is a positive transformation of the elasticity of intertemporal substitution. The discount factor is increasing in the magnitude, which is consistent with previous studies. The elasticity of intertemporal substitution is increasing in the magnitude, meaning that the rewards on the two dates are more substitutable to the subjects when the subjects face a larger total budget. This results in choices closer to the two corners (to which corner depends on whether $\delta R > 1$). Thereby, we verify Hypothesis 3 and Hypothesis 4.

¹⁵ For the other three pairs of non-adjacent magnitudes: the differences in β are not significant, while the differences in δ^τ and in α are all significant at the 1% level.

Table 5. Discounting and Curvature Parameter Estimates in the Aggregate-level Estimation with the CRRA specification

Model: Magnitude:	Tobit All	€20	€40	Tobit €60	€80	€160
Present bias: $\hat{\beta}$	0.989 (0.018)	0.989 (0.021)	0.989 (0.018)	0.986 (0.018)	0.997 (0.017)	0.986 (0.020)
Discount factor over four weeks: $\hat{\delta}^\tau$	0.968 (0.012)	0.948 (0.016)	0.961 (0.013)	0.971 (0.012)	0.972 (0.011)	0.982 (0.012)
CRRA curvature: $\hat{\alpha}$	0.955 (0.004)	0.928 (0.007)	0.947 (0.005)	0.952 (0.005)	0.958 (0.004)	0.968 (0.003)
S.e. of the error term: $\hat{\sigma}$	3.699 (0.281)	2.294 (0.200)	2.986 (0.245)	3.369 (0.269)	3.857 (0.307)	5.314 (0.454)
Log-likelihood	-13678.51			-13538.56		
Observations	7,105			7,105		
Uncensored	1,969			1,969		
Clusters	203			203		

Notes: Two-limit Tobit estimators. CRRA estimation with $\omega = 7.89$ (average reported background consumption). Column 1: assuming that parameters are invariant to magnitudes. Column 2-6: assuming that parameters vary with magnitudes. Clustered standard errors in parentheses. Log-likelihood has been corrected for the transformation of dependent variables. Standard errors calculated via the delta method.

Table 6. Estimates of Parameter Differences between Magnitudes in the CRRA specification

Magnitude:	€40 - €20	€60 - €40	€80 - €60	€160 - €80	€60 - €20	€80 - €40	€160 - €60
Present bias: $\hat{\beta}$	-0.000 (0.008)	-0.003 (0.006)	0.011* (0.006)	-0.011 (0.009)	-0.003 (0.010)	0.008 (0.008)	0.000 (0.009)
Discount factor over four weeks: $\hat{\delta}^\tau$	0.014** (0.006)	0.010* (0.005)	0.001 (0.005)	0.010** (0.004)	0.024*** (0.008)	0.011* (0.006)	0.011* (0.006)
CRRA curvature: $\hat{\alpha}$	0.018*** (0.003)	0.006*** (0.002)	0.006*** (0.002)	0.011*** (0.002)	0.024*** (0.004)	0.011*** (0.002)	0.016*** (0.002)
S.e. of the error term: $\hat{\sigma}$	0.692*** (0.106)	0.383*** (0.092)	0.487*** (0.108)	1.457*** (0.218)	1.075*** (0.138)	0.871*** (0.125)	1.944*** (0.258)

Notes: Estimates of parameter differences are inferred from the Two-limit Tobit estimation by the delta method. The estimation assumes CRRA utility with $\omega = 7.89$. Separate parameters are estimated for each magnitude among €20, €40, €60, €80 and €160. There are 1,421 observations (203 clusters) for each magnitude. Clustered standard errors in parentheses. Standard errors calculated via the delta method. ***, ** and * indicate significance at the 1 percent level, 5 percent level, and 10 percent level, respectively.

To get an idea about the relative importance of the two channels of the magnitude effect, we use the estimates above to predict choices in the 35 questions for both the Present Group and the Delayed Group. Table 7 presents the marginal effects of allowing one parameter to vary with the magnitude: in each row, we allow only one parameter, either δ or α , to vary with the magnitude of the decisions (as indicated by the column title), but fix the other two parameters at the value estimated from the magnitude of €20. Each number in a cell is the total change (in unit of $\frac{N_k}{100}$, the percentage of the total budget) in the seven decisions with the corresponding

magnitude. The results show that the marginal effect of allowing α to vary with the magnitude is about as large as the marginal effect of allowing δ to vary. This suggests that the magnitude effect on the elasticity of intertemporal substitution is at least as important as the magnitude effect on the discount rate.

Table 7. Marginal Effects of Allowing a Parameter to Vary with Magnitudes in the CRRA specification

	Magnitude:	€40	€60	€80	€160
Parameter	$\beta_1, \delta_k, \alpha_1$ (Delayed):	21.3	34.7	36.6	48.6
values used in	$\beta_1, \delta_k, \alpha_1$ (Present):	21.8	36.5	38.1	50.7
predicting:	$\beta_1, \delta_1, \alpha_k$ (Delayed):	24.8	33.9	44.4	65.5
	$\beta_1, \delta_1, \alpha_k$ (Present):	27.7	38.0	49.2	72.8

Notes: The changes in choices predicted by the CRRA Tobit model using the parameter values indicated by the row title compared with $(\beta_1, \delta_1, \alpha_1)$, for the two groups separately. k in the row titles stands for the magnitude in the column title. For instance, the first cell in the first row is the difference between the choices made in the seven decisions with the magnitude of €40 predicted by the model with parameter values $(\beta_1, \delta_k, \alpha_1)$ and those predicted by the model with parameter values $(\beta_1, \delta_1, \alpha_1)$. In other words, it is the marginal effect of allowing δ to vary with the magnitude from €20 to €40. The unit is 1% of the total budget.

B. Individual-level estimation

The aggregate-level estimation provides evidence of positive magnitude effects on the discount factor and on intertemporal substitutability. One may wonder whether these results also hold at the individual level. Indeed, we find a huge individual heterogeneity in choices. One concern is that, when testing the magnitude effect on the aggregate preferences, there might be a bias resulting from forcing all subjects to have the same preferences and the same distribution of noise. To deal with this concern, we also perform individual-level estimation and tests.

1. Estimation and testing procedure

We keep all the assumptions that underlie equation (1) except for β since it is not identified in individual-level estimations. We estimate the discount factor (δ) and the intertemporal substitutability (α) for each combination of subject and stake, and then test if the two parameters are increasing in the magnitude within-subject.

One important difference from the aggregate-level estimation is that there might be an under-identification problem when a subject made no or only one interior choice under a stake. Actually there are 627 out of 1015 (62%) combinations of subjects and stakes suffering from such a problem. We thereby adopt a conservative way to test the magnitude effect. First, we yield point estimates of δ and α if possible. Whenever there is an under-identification problem, we remove the error term from (1) and then infer the intervals of δ and α that can generate the observations. Second, we perform a one-tailed sign test on the two parameters, respectively, with the null hypotheses that they do not change with the magnitude. The sign test only requires that the distribution of a parameter does not differ between magnitudes, while it allows the distribution to be different across subjects. For a comparison between a point estimate and an interval estimate, we recognize a difference only if the point is not in the interior of the interval. For a comparison between two interval estimates, we recognize a difference if the two intervals do not overlap.

2. Results

Table 8 shows the results of the tests at the individual level. We reject the null hypotheses of no magnitude effect on the two parameters, in favor of positive magnitude effects. This shows that the two channels of the magnitude effect on intertemporal choices are robust against individual heterogeneity.

Table 8. Sign tests on preference parameters between magnitudes

		€20 ~ €40	€40 ~ €60	€60 ~ €80	€80 ~ €160	€20 ~ €60	€40 ~ €80	€60 ~ €160
Discount factor over four weeks: δ^T	#increase	63/98/42	62/103/38	52/114/37	47/117/39	73/97/33	69/101/33	59/109/35
	/unchanged /decrease p-value	0.0255**	0.0107**	0.0687*	0.2253	0.0001***	0.0003***	0.0086***
CRRA curvature: $\hat{\alpha}$	#increase	61/123/19	57/122/24	48/128/27	55/130/18	62/125/16	59/124/20	58/127/18
	/unchanged /decrease p-value	0.0000***	0.0002***	0.0101**	0.0000***	0.0000***	0.0000***	0.0000***

Notes: Right-tailed sign tests on the differences of parameters between two magnitudes. 203 sets of observations for each magnitude. $\omega = 7.888$. When under-identification occurs, interval estimates are yielded for the two

preference parameters. A point estimate and an interval estimate are considered as different only if the point is not in the interior of the interval. Two interval estimates are considered as different only if their intersection is empty or a singleton. ***, ** and * indicate significance at the 1 percent level, 5 percent level, and 10 percent level, respectively.

V. Interpretations

The results above imply that when an average subject faces a larger budget in an intertemporal allocation task, she behaves more patiently, but also she regards rewards to be more substitutable between dates.

A. Relation with the magnitude effect on risk aversion

According to the Discounted Expected Utility (DEU) theory, the risk attitude and the elasticity of intertemporal substitution are represented by the same parameter, since risk aversion and imperfect fungibility both originate from diminishing marginal utility. Therefore, one may wonder whether the magnitude effect on intertemporal substitutability is the same as the magnitude effect on risk attitudes.

We find evidence against this equivalence. Holt and Laury (2002) investigated the magnitude effect on risk attitudes with Multiple Price List (MPL) questions. They found a significant, positive magnitude effect: when faced with a larger magnitude, people appear to be more risk averse in terms of the relative risk aversion. This is in the opposite direction as the effect we find. Their finding suggests an increase in the concavity as the magnitude increases while ours shows a movement towards linearity. This contradiction suggests that the magnitude effect on relative risk aversion is not driving the magnitude effect on intertemporal substitutability.

Some other studies also suggest that risk aversion and intertemporal substitutability should be separated. Andreoni and Sprenger (2012) found no significant correlation at the individual level between the curvature estimated by the CTB method and the risk attitude elicited by the MPL method. Abdellaoui et al.

(2013), Miao and Zhong (2015) and Cheung (2015) also found that the utility curvature elicited from intertemporal tasks is different from that elicited from tasks with risk. We provide evidence from a different perspective: while the previous studies showed that the degrees of concavity are different for the two kinds of utility functions, we show that the degrees of concavity change in opposite directions when the stake is varied.

This finding has implications for both theories and experimental methods. First, it lends support to the theories which separate intertemporal substitutability from risk aversion, such as Epstein and Zin (1989). Second, it casts doubt on the use of a risk-elicitation task to correct for the curvature when eliciting time preferences.

B. Relation with borrowing constraints

In theory, a binding borrowing constraint can lead to a magnitude effect on the monetary discount rate in a single-reward task if the background consumption is expected to grow over time, as shown by Epper (2015). However, Meier and Sprenger (2010) found that experimentally elicited long-run discount rates are uncorrelated with credit constraints, suggesting that on average, whether the borrowing constraint is binding does not affect intertemporal choices in experiments.

Moreover, given the fact that subjects may have savings which provide limited liquidity, the fraction of subjects whose borrowing constraints are binding is increasing in the stake. For this reason, if the borrowing constraint is a main issue, we should observe that the intertemporal substitutability is decreasing in the stake, which is inconsistent with our results. Therefore, we believe that a binding borrowing constraint is not the main driver of our results.

C. Relation with existing theories

We discuss the implications of our empirical findings for some theories that may explain the magnitude effect on intertemporal choices.

One model that can account for the magnitude effect on the discount factor was proposed by Benhabib et al. (2010). They developed a model with a fixed cost of delaying rewards. The idea is that whenever a delayed reward is chosen, a fixed cost is incurred, so that as the stake increases, the cost becomes relatively less important and hence the subject appears more patient.

Noor (2011) proposed a model of magnitude-dependent discounting, which leads to similar predictions. In his model, the discount function is increasing in the utility at the later period. As the stake gets larger, the discount function converges to 1.¹⁶

One theory that can explain the magnitude effect on intertemporal substitutability is an extended version of the dual-self bank-nightclub model of Fudenberg and Levine (2006). In the original model, the agent maximizes her lifelong utility by choosing the amount of pocket cash when no temptation is present and choosing the amount of consumption when a windfall is available and temptation plays a role. The agent will use up a small windfall since the self-control cost lowers the utility of savings. A small windfall is not integrated with the lifelong wealth. As a result, the utility function for windfalls is much more concave when the size of the windfall is below a certain threshold than when it is above the threshold.

The model can explain a magnitude effect on intertemporal substitutability if we impose the assumption that an agent who anticipates a reward in the future does not immediately adjust her cash allocation plan. Instead, she keeps the anticipated reward in the mental account of windfalls until it is received and part of it is

¹⁶ Both models can explain the magnitude effect on the monetary discount rate in single-reward tasks. When they are applied to the intertemporal allocation tasks, both of them predict a jump from the sooner corner to an interior point or to the later corner in every wealth expansion path. Unfortunately we are not able to confirm nor reject the existence of such jumps; since we have only five points on each wealth expansion path, it is hard to distinguish jumps from curves.

consumed. Only after the remainder is moved into the mental account of savings does she reschedule her future consumption.

Using this assumption the model predicts that a subject will tend to make interior choices when the budget is small, i.e., below the threshold induced by the self-control costs. Since the utility function for windfalls is very concave the subject balances extra consumption on the sooner date and on the later date. As the budget increases above the threshold, the subject will want to save part of it for consumption smoothing. Since the utility function for savings is much less concave (close to linear) these savings will be allocated fully to either the sooner date (when the interest rate is small) or the later date (when the interest rate is large). Hence, as the budget increases the intertemporal substitutability increases and it will appear as if the utility function has become less concave (see Appendix C for a simulation).

Another model that can explain the magnitude effect on intertemporal substitutability is the mental zooming theory proposed by Holden (2014). The theory presumes that people integrate more background consumption with the experimental reward as the size of the reward increases. If the budget increases, individuals 'zoom out' as it were, and take a broader perspective in the decision problem. One reason may be that individuals are likely to divide and use up a bigger windfall over a longer time period. Based on the data collected from his field experiment with Malawian peasants, Holden showed that the magnitude effect on time preferences in single-reward tasks would disappear if the unobserved background consumption is assumed to be an increasing function of the stake.

In intertemporal allocation tasks, the increasing background consumption can generate a magnitude effect on intertemporal substitutability. To see why, denote the observed elasticity of intertemporal substitution in experimental rewards by e_z . The relationship between e_z and preference parameters is

$$e_z = \frac{1}{1 - \alpha} \cdot \frac{\log\left(\frac{Z_{t+\tau}}{Z_t}\right)}{\log\left(\frac{Z_{t+\tau} + \omega}{Z_t + \omega}\right)}.$$

Since e_z is increasing in both α and ω , an increase in α and an increase in ω are competing explanations for the magnitude effect on intertemporal substitutability. If subjects take into account more background consumption as the total budget increases, we would observe a greater sensitivity to the interest rate, i.e. a greater e_z . When we assume a fixed background consumption, however, the pattern will be attributed to a magnitude effect on α .

Another characteristic of the mental zooming theory is that it allows wealth expansion paths to be downward-sloping, as we observe for a large proportion of our subjects (See Appendix D).

Both the mental-accounting Fudenberg-Levine model and the mental zooming theory point to partial integration with lifelong wealth, which seems to be an important mechanism of the magnitude effect on intertemporal substitutability between rewards. Andersen et al. (2012) showed empirically that subjects only partially integrate experimental rewards with wealth in risk preference tasks. While they provide evidence of partial asset integration by exploiting variation in personal wealth, our results suggest that the degree of asset integration is increasing in the stake by providing within-subject evidence.

None of the current models can explain both a magnitude effect on the discount factor and a magnitude effect on the intertemporal substitutability. Of course, the two channels can be explained by a mode-switching model in which individuals are assumed to have different preferences for different stakes. However, a truly unified explanation is still lacking.

VI. Conclusion

Our study investigates the magnitude effect on intertemporal choices in a recently-introduced task, the intertemporal allocation task. After adapting the concept for the new task, we verify the existence of a magnitude effect. The magnitude effect exists even when both rewards are only available in the future, implying that the factor which drives the present bias cannot fully account for the magnitude effect.

We then look deeper into the effect, by exploring the channels. The results underscore the importance of a dimension which is often overlooked, namely, the intertemporal substitutability. We find evidence that both the discount factor and the intertemporal substitutability change with the magnitude of rewards.

Some existing theories may provide explanations for one of the two channels. A cost-of-delay model (Benhabib et al., 2010) or a magnitude-dependent discounting model (Noor, 2011) can account for a magnitude effect on the discount factor. Models which allow the degree of asset integration (mental accounting) to vary with the size of the budget can explain a magnitude effect on intertemporal substitutability. However, a new theory would be needed to account for both channels simultaneously and in a unified way.

For the magnitude effect on intertemporal substitutability, existing theories tend to attribute it to the varying degree of asset integration, however, sharper tests are needed to check the conjecture and to explore specific factors. One possible way would be to restrict the dates on which rewards can be consumed and then to check if the restriction has an effect on intertemporal choices.

REFERENCES

- Abdellaoui, M., Bleichrodt, H., l'Haridon, O., & Paraschiv, C. (2013). Is There One Unifying Concept of Utility? An Experimental Comparison of Utility Under Risk and Utility Over Time. *Management Science*, 59(9), pp. 2153–2169.
- Andersen, S., Cox, J. C., Harrison, G. W., Lau, M., Rutström, E. E., & Sadiraj, V. (2012). Asset Integration and Attitudes to Risk: Theory and Evidence. *Working Paper 2012-07*.
- Andersen, S., Harrison, G. W., Lau, M. I., & Rutström, E. E. (2011). Discounting Behavior and the Magnitude Effect. *Working Paper 2011-05*.
- Andersen, S., Harrison, G. W., Lau, M. I., & Rutström, E. E. (2013). Discounting Behavior and the Magnitude Effect: Evidence from a Field Experiment in Denmark. *Economica*, 80(320), pp. 670-697.
- Andreoni, J., & Sprenger, C. (2012). Estimating Time Preferences from Convex Budgets. *American Economic Review*, 102(7), pp. 3333-3356.
- Andreoni, J., Kuhn, M. A., & Sprenger, C. (2013). On Measuring Time Preferences. *NBER Working Paper No. 19392*.
- Augenblick, N., Niederle, M., & Sprenger, C. (2015). Working Over Time: Dynamic Inconsistency in Real Effort Tasks. *Quarterly Journal of Economics*, 130(3), 1067-1115.
- Benhabib, J., & Bisin, A. (2005). Modeling Internal Commitment Mechanisms and Self-Control: A Neuroeconomics Approach to Consumption-Saving Decisions. *Games and Economic Behavior*, 52(2), pp. 460-492.
- Benhabib, J., Bisin, A., & Schotter, A. (2010). Present-Bias, Quasi-Hyperbolic Discounting, and Fixed Costs. *Games and Economic Behavior*, 69(2), pp. 205-223.

- Chakraborty, A., Calford, E. M., Fenig, G., & Halevy, Y. (2014). Estimating Time Preferences from Convex Budgets: Comment on Normality and Impatience. *Working Paper 2014-08-07*.
- Cheung, S. L. (2015). Eliciting Utility Curvature and Time Preference. *Working Paper 2015-01*.
- Epper, T. (2015). Income Expectations, Limited Liquidity, and Anomalies in Intertemporal Choice. *University of St. Gallen, School of Economics and Political Science Discussion Paper No. 2015-19*.
- Epstein, L. G., & Stanley, E. Z. (1989). Substitution, Risk Aversion, and the Temporal Behavior of Consumption and Asset Returns: A Theoretical Framework. *Econometrica*, 57(4), pp. 937-969.
- Fischbacher, U. (2007). z-Tree: Zurich Toolbox for Ready-Made Economic Experiments. *Experimental Economics*, 10(2), 171-178.
- Frederick, S., Loewenstein, G., & O'Donoghue, T. (2002). Time Discounting and Time Preference: A Critical Review. *Journal of Economic Literature*, 40(2), pp. 351-401.
- Fudenberg, D., & Levine, D. K. (2006). A Dual-Self Model of Impulse Control. *American Economic Review*, 96(5), pp. 1449-1476.
- Fudenberg, D., & Levine, D. K. (2011). Risk, Delay, and Convex Self-Control Costs. *American Economic Journal: Microeconomics*, 3(3), pp. 34-68.
- Halevy, Y. (2015). Time Consistency: Stationarity and Time Invariance. *Econometrica*, 83(1), pp. 335-352.
- Holcomb, J. H., & Nelson, P. S. (1992). Another Experimental Look at Individual Time Preference. *Rationality and Society*, 4(2), 199-220.
- Holden, S. T. (2014). Explaining Anomalies in Intertemporal Choices: A Mental Zooming Theory. *Center for Land Tenure Studies Working Paper 02/14*. Norwegian University of Life Sciences (NMBU).

- Holt, C. A., & Laury, S. K. (2002). Risk Aversion and Incentive Effects. *American Economic Review*, 92(5), pp. 1644-1655.
- Keren, G., & Roelofsma, P. (1995). Immediacy and Certainty in Intertemporal Choice. *Organizational Behavior and Human Decision Processes*, 63(3), pp. 287-297.
- Kirby, K. N. (1997). Bidding on the Future: Evidence against Normative Discounting of Delayed Rewards. *Journal of Experimental Psychology: General*, 126, pp. 54-70.
- Kirby, K. N., Petry, N. M., & Bickel, W. K. (1999). Heroin Addicts Have Higher Discount Rates for Delayed Rewards. *Journal of Experimental Psychology: General*, 128, pp. 78-87.
- Meier, S., & Sprenger, C. (2010). Present-Biased Preferences and Credit Card Borrowing. *American Economic Journal: Applied Economics*, 2(1), pp. 193-210.
- Miao, B., & Zhong, S. (2015). Comment on “Risk Preferences Are Not Time Preferences”: Separating Risk and Time Preference. *American Economic Review*, 105(7), pp. 2272-86.
- Noor, J. (2011). Intertemporal Choice and the Magnitude Effect. *Games and Economic Behavior*, 72(1), pp. 255-270.
- Oja, H., & Randles, R. H. (2004). Multivariate Nonparametric Tests. *Statistical Science*, 19(4), pp. 598-605.
- Read, D. (2001). Is Time-Discounting Hyperbolic or Subadditive? *Journal of Risk and Uncertainty*, 23(1), pp. 5-32.
- Thaler, R. (1981). Some Empirical Evidence on Dynamic Inconsistency. *Economics Letters*, 8(3), pp. 201-207.

Appendix A. Parametric Analysis with Different Assumptions on ω

We check the sensitivity of the parameter estimates (Table A 1) and the magnitude effects (Table A 2) to alternative assumptions on the background consumption. The results show that the magnitude effects on the discount factor and on intertemporal substitutability are robust, except when ω is assumed to be very small. However, with a small background consumption, one should rarely make corner choices due to the motivation of consumption smoothing. But we do observe many corner choices in our sample. Hence, the case with a small ω is unlikely to be true.

Table A 1. Background Consumption, Parameter Estimates and Likelihood

Model: Magnitude:		Tobit All	€20	€40	Tobit €60	€80	€160
$\omega = 7.89$	Present bias: $\hat{\beta}$	0.989 (0.018)	0.989 (0.021)	0.989 (0.018)	0.986 (0.018)	0.997 (0.017)	0.986 (0.020)
	Discount factor over four weeks: $\hat{\delta}^{\tau}$	0.968 (0.012)	0.948 (0.016)	0.961 (0.013)	0.971 (0.012)	0.972 (0.011)	0.982 (0.012)
	CRRRA curvature: $\hat{\alpha}$	0.955 (0.004)	0.928 (0.007)	0.947 (0.005)	0.952 (0.005)	0.958 (0.004)	0.968 (0.003)
	S.e of the error term: $\hat{\sigma}$	3.699 (0.281)	2.294 (0.200)	2.986 (0.245)	3.369 (0.269)	3.857 (0.307)	5.314 (0.454)
	Log-likelihood	-13678.51			-13538.56		
$\omega = \max(\omega_i, 0.01)$	Present bias: $\hat{\beta}$	0.990 (0.018)	0.989 (0.020)	0.990 (0.018)	0.987 (0.015)	0.998 (0.017)	0.987 (0.022)
	Discount factor over four weeks: $\hat{\delta}^{\tau}$	0.968 (0.012)	0.948 (0.014)	0.962 (0.012)	0.971 (0.011)	0.972 (0.011)	0.982 (0.012)
	CRRRA curvature: $\hat{\alpha}$	0.962 (0.004)	0.945 (0.006)	0.956 (0.005)	0.960 (0.004)	0.964 (0.004)	0.972 (0.003)
	S.e of the error term: $\hat{\sigma}$	4.422 (0.358)	3.048 (0.300)	3.716 (0.315)	4.102 (0.328)	4.570 (0.370)	6.115 (0.552)
	Log-likelihood	-13920.57			-13815.64		
$\omega = 12.89$	Present bias: $\hat{\beta}$	0.989 (0.019)	0.989 (0.019)	0.989 (0.018)	0.985 (0.019)	0.997 (0.019)	0.986 (0.020)
	Discount factor over four weeks: $\hat{\delta}^{\tau}$	0.969 (0.012)	0.948 (0.014)	0.962 (0.013)	0.972 (0.012)	0.973 (0.012)	0.982 (0.012)
	CRRRA curvature: $\hat{\alpha}$	0.945 (0.005)	0.903 (0.010)	0.932 (0.007)	0.940 (0.006)	0.948 (0.005)	0.963 (0.004)
	S.e of the error term: $\hat{\sigma}$	3.013 (0.229)	1.697 (0.147)	2.329 (0.187)	2.698 (0.214)	3.144 (0.262)	4.490 (0.384)
	Log-likelihood	-13623.90			-13443.15		
$\omega = \omega_i + 5$	Present bias: $\hat{\beta}$	0.990 (0.018)	0.990 (0.019)	0.990 (0.019)	0.987 (0.019)	0.998 (0.019)	0.987 (0.019)
	Discount factor over four weeks: $\hat{\delta}^{\tau}$	0.968 (0.012)	0.948 (0.014)	0.962 (0.013)	0.971 (0.012)	0.972 (0.013)	0.982 (0.012)
	CRRRA curvature: $\hat{\alpha}$	0.948 (0.005)	0.912 (0.009)	0.936 (0.006)	0.944 (0.006)	0.951 (0.005)	0.964 (0.004)
	S.e of the error term: $\hat{\sigma}$	3.213 (0.246)	1.870 (0.163)	2.529 (0.205)	2.899 (0.231)	3.355 (0.275)	4.430 (0.398)
	Log-likelihood	-13693.32			-13527.03		
$\omega = 0.01$	Present bias: $\hat{\beta}$	0.990 (0.019)	0.990 (0.020)	0.990 (0.019)	0.987 (0.018)	0.998 (0.020)	0.986 (0.022)
	Discount factor over four weeks: $\hat{\delta}^{\tau}$	0.964 (0.013)	0.945 (0.015)	0.959 (0.013)	0.968 (0.010)	0.969 (0.013)	0.980 (0.011)
	CRRRA curvature: $\hat{\alpha}$	0.989 (0.001)	0.989 (0.001)	0.989 (0.001)	0.989 (0.001)	0.989 (0.001)	0.989 (0.001)
	S.e of the error term: $\hat{\sigma}$	15.333 (1.216)	14.511 (1.239)	14.500 (1.154)	14.470 (1.115)	15.251 (1.220)	17.613 (1.478)
	Log-likelihood	-15596.78			-15575.13		
Observations		7,105			7,105		
Uncensored		1,969			1,969		
Clusters		203			203		

Notes: Two-limit Tobit estimators. Panel 1: ω = average reported background consumption. Panel 2: ω = individual reported background consumption (except for one subject, we replace the zero consumption with 0.01). Panel 3: ω = average reported background consumption plus the participation fee. Panel 4: ω = individual background consumption plus the participation fee. Panel 5: $\omega = 0.01$. Column 1: assuming that parameters are invariant to magnitudes. Column 2-

6: assuming that parameters vary with magnitudes. Clustered standard errors in parentheses. Log-likelihood has been corrected for the transformation of dependent variables. Standard errors calculated via the delta method.

Table A 2. Background Consumption and Magnitude Effects

	Magnitude:	€40 - €20	€60 - €40	€80 - €60	€160 - €80	€60 - €20	€80 - €40	€160 - €60
$\omega = 7.89$	Present bias: $\hat{\beta}$	-0.000	-0.003	0.011*	-0.011	-0.003	0.008	0.000
	Discount factor over four weeks: δ^τ	0.014**	0.010*	0.001	0.010**	0.024***	0.011*	0.011*
	CRRRA curvature: $\hat{\alpha}$	0.018***	0.006***	0.006***	0.011***	0.024***	0.011***	0.016***
	S.e of the error term: $\hat{\sigma}$	0.692***	0.383***	0.487***	1.457***	1.075***	0.871***	1.944***
$\omega = \max(\omega_i, 0.01)$	Present bias: $\hat{\beta}$	0.001	-0.003	0.011*	-0.011	-0.002	0.008	-0.000
	Discount factor over four weeks: δ^τ	0.013**	0.010**	0.001	0.010**	0.023***	0.010*	0.011*
	CRRRA curvature: $\hat{\alpha}$	0.011***	0.004**	0.004***	0.008***	0.015***	0.008***	0.012***
	S.e of the error term: $\hat{\sigma}$	0.668***	0.386***	0.468***	1.545***	1.054***	0.854***	2.013***
$\omega = 12.89$	Present bias: $\hat{\beta}$	-0.000	-0.003	0.011*	-0.011	-0.003	0.008	0.000
	Discount factor over four weeks: δ^τ	0.014***	0.010**	0.001	0.010**	0.024***	0.011*	0.011*
	CRRRA curvature: $\hat{\alpha}$	0.028***	0.009***	0.008***	0.014***	0.037***	0.017***	0.022***
	S.e of the error term: $\hat{\sigma}$	0.632***	0.370***	0.446***	1.346***	1.002***	0.815***	1.792***
$\omega = \omega_i + 5$	Present bias: $\hat{\beta}$	0.001	-0.003	0.011*	-0.011	-0.002	0.008	-0.000
	Discount factor over four weeks: δ^τ	0.014***	0.010*	0.001	0.010**	0.023***	0.011*	0.011*
	CRRRA curvature: $\hat{\alpha}$	0.025***	0.008***	0.007***	0.013***	0.032***	0.015***	0.020***
	S.e of the error term: $\hat{\sigma}$	0.659***	0.371***	0.456***	1.374***	1.030***	0.827***	1.830***
$\omega = 0.01$	Present bias: $\hat{\beta}$	-0.000	-0.002	0.010	-0.011	-0.003	0.008	-0.001
	Discount factor over four weeks: δ^τ	0.014**	0.009	0.001	0.011**	0.023***	0.010	0.011*
	CRRRA curvature: $\hat{\alpha}$	0.000	-0.000	0.001	0.001***	-0.000	0.000	0.002***
	S.e of the error term: $\hat{\sigma}$	-0.011	-0.030	0.781*	2.362***	-0.041	0.751	3.143***

Notes: Estimates of parameter differences are inferred from the Two-limit Tobit estimation by the delta method. Parameters are separately set for each magnitude among €20, €40, €60, €80 and €160. Panel 1: ω = average reported background consumption. Panel 2: ω = individual reported background consumption (except for one subject, we replace the zero consumption with 0.01). Panel 3: ω = average reported background consumption plus the participation fee. Panel 4: ω = individual background consumption plus the participation fee. Panel 5: $\omega = 0.01$. There are 1,421 observations (203 clusters) for each magnitude. Clustered standard errors in parentheses. Standard errors calculated via the delta method. ***, ** and * indicate significance at the 1 percent level, 5 percent level, and 10 percent level, respectively.

Appendix B. Parametric Analysis with Estimation of ω

The CRRA specification with exogenously set ω is simple and easy to estimate, however, one may suspect that the average self-reported background consumption does not match the true background consumption integrated with the experimental rewards in decision making, or the utility function is misspecified. In particular, if the utility function is Hyperbolic Absolute Risk Aversion (HARA), ω in (1) does

not represent the background consumption but is a mixture of the background consumption and the HARA intercept parameter.

In order to meet the challenges above, we estimate ω instead of setting it exogenously. By doing this, we “let the data tell” what values are suitable for ω , and we can also identify the magnitude effects on ω .

1. Model

We assume a normally distributed error term at the choice level. The error term can be arisen either because of idiosyncratic shocks in preference or because of imprecision in placing the scrollbar.¹⁷ In specific, a latent choice is

$$z_{t;i,j,k}^* = \begin{cases} \frac{m - \left((\beta \delta R_j)^{\frac{1}{1-\alpha}} - 1 \right) \omega}{R + (\beta \delta R_j)^{\frac{1}{1-\alpha}}} + \epsilon_{i,j,k}, & \text{if } t = 0 \\ \frac{m - \left((\delta R_j)^{\frac{1}{1-\alpha}} - 1 \right) \omega}{R + (\delta R_j)^{\frac{1}{1-\alpha}}} + \epsilon_{i,j,k}, & \text{if } t > 0 \end{cases}, \epsilon_{i,j,k} \sim N\left(0, \frac{\sigma_k}{100} m\right).$$

Then the choices are censored at the two corners so that the observed choices are

$$z_{t;i,j,k} = \begin{cases} 0, & \text{if } z_{t;i,j,k}^* \leq 0 \\ z_{t;i,j,k}^*, & \text{if } 0 < z_{t;i,j,k}^* < \frac{m_k}{R_j} \\ \frac{m_k}{R_j}, & \text{if } z_{t;i,j,k}^* \geq \frac{m_k}{R_j} \end{cases}.$$

This is a two-limit nonlinear censored model, which can be estimated by the quasi-maximum likelihood method.

¹⁷ In the model with a normally distributed error term additive to the log allocation ratio, the estimator of ω is nonlinear in the error term. Simulation shows that the estimator of ω is severely biased given our sample size, though it is asymptotically consistent. The model we assume here is the same as the one implicitly assumed by Andreoni and Sprenger (2012) when they perform the nonlinear least square (NLS) estimation. The difference is that we employ the quasi-maximum likelihood estimation, by which we take into account censoring.

2. Results

Table A 3 reports the estimates of the parameters from the specifications with magnitude-invariant parameters and with magnitude-specific parameters, respectively.

Table A 4 presents the estimates of the parameter differences between magnitudes. We find a significant magnitude effect on the discount rate. The magnitude effect on the exponent parameter, α , is only significant between the magnitudes of €20 and €40. This is reasonable since we find a strong magnitude effect on the background consumption and the HARA intercept parameter (i.e. ω), which have explained most of the magnitude effects on intertemporal substitutability.

Table A 3. Discounting and Curvature Parameter Estimates in the Aggregate-level Estimation with the HARA specification

Model:	Nonlinear Censored	Nonlinear Censored				
Magnitude:	All	€20	€40	€60	€80	€160
Present bias: $\hat{\beta}$	1.000 (0.004)	0.994 (0.010)	1.002 (0.005)	1.000 (0.005)	1.000 (0.005)	0.997 (0.005)
Discount factor over four weeks: $\hat{\delta}^T$	0.953 (0.004)	0.952 (0.008)	0.960 (0.003)	0.962 (0.003)	0.965 (0.004)	0.967 (0.004)
Curvature parameter: $\hat{\alpha}$	0.997 (0.003)	0.964 (0.009)	0.984 (0.006)	0.982 (0.004)	0.984 (0.003)	0.986 (0.002)
Background consumption and HARA intercept: $\hat{\omega}$	9.556 (2.046)	10.713 (2.209)	15.790 (3.364)	22.551 (4.461)	30.834 (5.896)	67.814 (12.598)
S.d. of the error term: $\hat{\sigma}$	63.948 (4.843)	78.491 (6.835)	68.110 (5.468)	62.147 (4.771)	62.426 (5.010)	65.890 (5.700)
Log-likelihood	-13136.00			-12836.01		
Observations	7,105			7,105		
Uncensored	1,969			1,969		
Clusters	203			203		

Notes: Quasi-Maximum Likelihood estimators. Column 1: assuming that parameters are the same across magnitudes. Column 2-6: assuming that parameters vary with magnitudes. Clustered standard errors in parentheses. Standard errors calculated via the delta method.

Table A 4. Estimates of Parameter Differences between Magnitudes in the HARA specification

Magnitude:	€40 - €20	€60 - €40	€80 - €60	€160 - €80	€60 - €20	€80 - €40	€160 - €60
Present bias: $\hat{\beta}$	0.00807 (0.00552)	-0.00180 (0.00206)	0.00038 (0.00241)	-0.00303 (0.00249)	0.00572 (0.00578)	-0.00197 (0.00255)	-0.00265 (0.00296)
Discount factor over four weeks: $\hat{\delta}^{\overline{r}}$	0.00734 (0.00570)	0.00252* (0.00138)	0.00258 (0.00169)	0.00267* (0.00141)	0.00986* (0.00574)	0.00510*** (0.00176)	0.00526** (0.00215)
Curvature parameter: $\hat{\alpha}$	0.02007*** (0.00737)	0.00097 (0.00370)	0.00192 (0.00225)	-0.00219 (0.00170)	0.01827** (0.00726)	0.00012 (0.00459)	0.00411 (0.00269)
Background consumption and HARA intercept: $\hat{\omega}$	5.077*** (1.959)	6.761*** (1.992)	8.283*** (2.471)	36.981*** (7.505)	11.838*** (2.932)	15.044*** (3.551)	45.263*** (8.805)

Notes: Estimates of parameter differences are inferred from the quasi-maximum likelihood estimates by the delta method. Parameters are separately set for each magnitude among €20, €40, €60, €80 and €160. There are 1,421 observations (203 clusters) for each magnitude. Clustered standard errors in parentheses. Standard errors calculated via the delta method. ***, ** and * indicate significance at the 1 percent level, 5 percent level, and 10 percent level, respectively.

As we do for the CRRA specification, we use the estimates to predict choices in the 35 questions for both the Present Group and the Delayed Group. Table A 5 presents the marginal effects of allowing parameters to vary with the magnitude: in Row 1-2, we allow δ to vary with the magnitude of the decisions (as indicated by the column title), but control other two parameters to be the value estimated from the magnitude of €20; in Row 3-4, we allow α and ω to vary with the magnitude. The results show that the marginal effect of allowing α and ω to vary with the magnitude is comparable with the marginal effect of allowing δ to vary. It is consistent with our finding in the Tobit estimation, which implies that the magnitude effect on intertemporal substitutability is at least equally important as the magnitude effect on the discount rate.

Table A 5. Marginal Effects of Allowing a Parameter to Vary with Magnitudes in the HARA specification

Magnitude:	€40	€60	€80	€160
Parameter $\beta_1, \delta_k, \alpha_1, \omega_1$ (Delayed):	13.9	22.0	25.9	28.0
values used in $\beta_1, \delta_k, \alpha_1, \omega_1$ (Present):	17.3	22.1	26.0	28.1
predicting: $\beta_1, \delta_1, \alpha_k, \omega_k$ (Delayed):	8.5	19.4	26.1	36.5
$\beta_1, \delta_1, \alpha_k, \omega_k$ (Present):	10.9	21.7	29.0	40.2

Notes: The changes in choices predicted by the HARA nonlinear censored model using the parameter values indicated by the row title compared with $(\beta_1, \delta_1, \alpha_1, \omega_1)$, for the two groups separately. k in the row titles stands for the magnitude in the column title. For instance, the first cell in the first row is the difference between the choices made in the seven decisions with the magnitude of €40 predicted by the model with parameter values

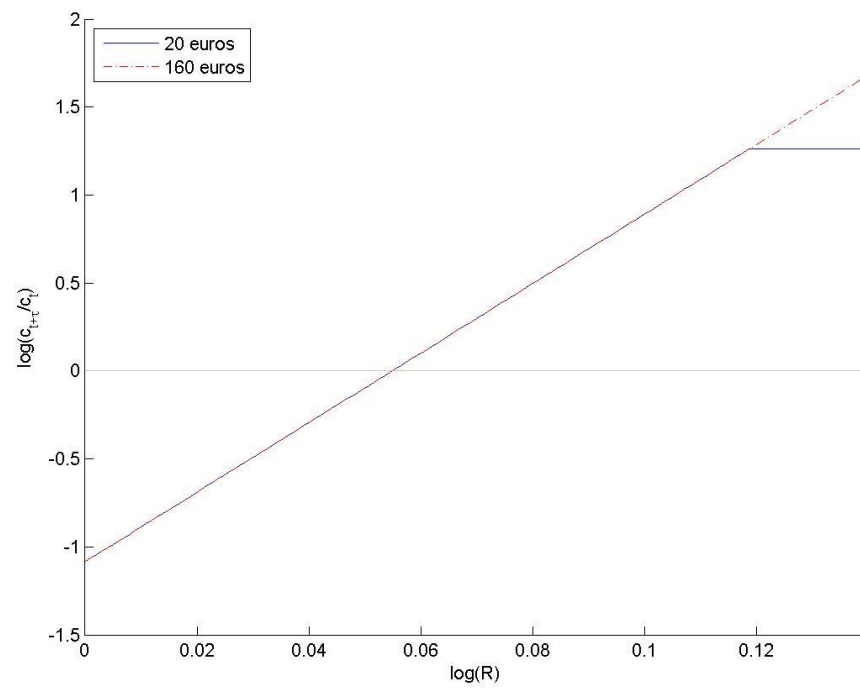
$(\beta_1, \delta_k, \alpha_1, \omega_1)$ and those predicted by the model with parameter values $(\beta_1, \delta_1, \alpha_1, \omega_1)$. In other words, it is the marginal effect of allowing δ to vary with the magnitude from €20 to €40.

Appendix C. A Simulation of the Mental-Accounting Fudenberg-Levine Model

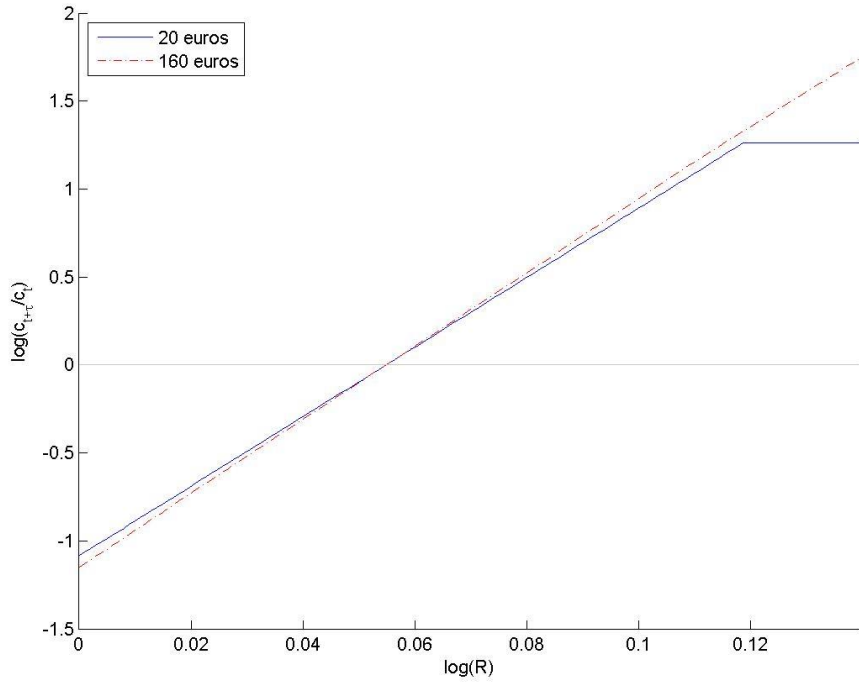
We perform a simulation according to the mental-accounting Fudenberg-Levine model described in Section V. Figure A 1 shows how the dependent variable in our Tobit estimation, $\ln\left(\frac{z_t + \omega}{z_{t+\tau} + \omega}\right)$, changes with the independent variable, $\ln R$.

Figure A 1(a) displays the curves in case the true model is specified by equation (1). Note that the slope of the curve (when it is not censored) is $\frac{1}{1-\alpha}$. Thus a greater slope stands for a larger intertemporal substitutability. The horizontal-axis intercept is $-\ln \delta$. Thus a greater horizontal-axis intercept stands for a smaller discount factor.

Figure A 1(b) displays the curves in case the true model is the mental-accounting Fudenberg-Levine model. When the budget is €20, both rewards are taken as pocket cash, so the curve is a straight line, as same as predicted by (1). However, when the budget gets larger, the curve is with a greater slope. Therefore, we observe a positive relation between intertemporal substitutability and the size of budget. On the other hand, the horizontal-axis intercept does not change with the stake, suggesting that the model cannot explain magnitude effect on the discount factor.



(a) Prediction of the model specified by equation (1)



(b) Prediction of the extended Fudenberg-Levine model

Figure A 1. Simulated relationships between the dependent variable and the independent variable of the Tobit estimation

Appendix D. Downward-Sloping Wealth Expansion Path

The results presented in Section IV and in Appendix B show that the data cannot be explained by the discounted utility theory with a HARA atemporal utility function in which all parameters are invariant to the magnitude. One may suspect if the increasing exponent implies that the data might be explained by the discounted utility theory with a DRRA utility function which is *not* HARA. In this Appendix, we show an interesting individual-level pattern in our data which may provide a clue.

The pattern we have particular interest in is the downward sloping wealth expansion path. It refers to the situation in which a subject allocates less money to

the sooner date for a larger magnitude than for a smaller magnitude but with the same interest rate. Formally it refers to a pair of sooner rewards in Part I, $(z_{t;i,j,k}, z_{t;i,j,k+1})$, made by the same subject, i ($i = 1, \dots, 203$), under the same interest rate, R_j ($j = 1, \dots, 7$), and consecutive magnitudes, $m_k < m_{k+1}$ ($k = 1, \dots, 4$), such that $z_{t;i,j,k} > z_{t;i,j,k+1}$. Chakraborty et al. (2014) called it wealth monotonicity violation. Figure A 2 gives an example of a downward sloping expansion path.

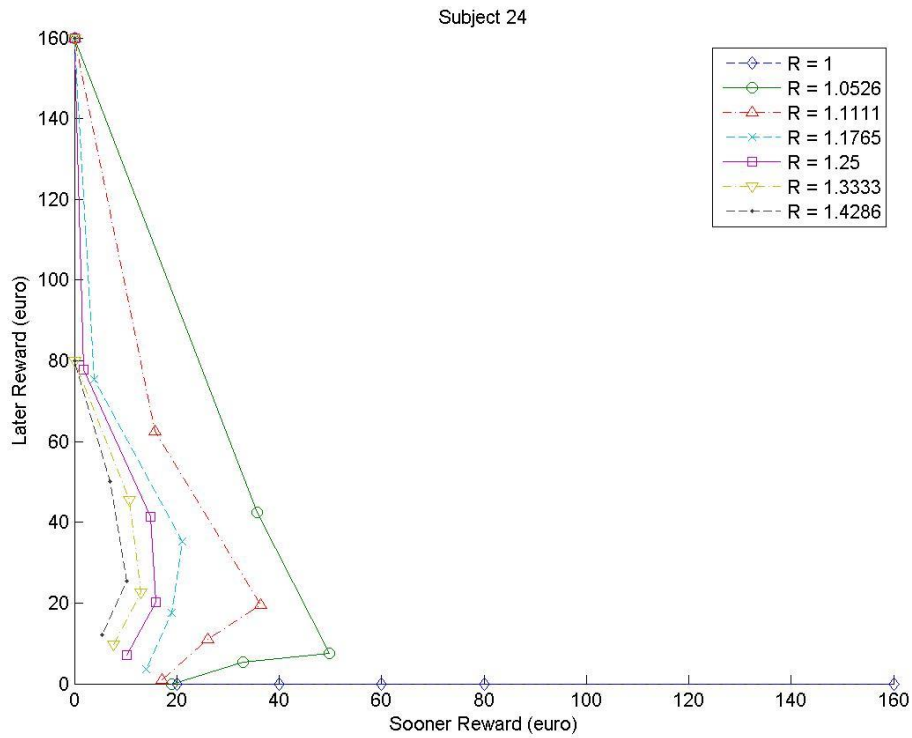


Figure A 2. An Example of a Downward Sloping Wealth Expansion Path

The downward-sloping expansion path is interesting because it cannot be rationalized by any additively separable preference with non-decreasing, weakly concave atemporal utility functions and magnitude-independent discounting. To see why, consider a subject who has allocated a few tokens to the sooner reward and the later reward. If she is given more tokens to allocate, she will allocate only

new tokens but never remove allocated tokens from a date, since the marginal utility of a new token can never be higher than that of an allocated token.

We found 99 subjects (48.8%) who presented at least one segment of downward sloping expansion path, among which 92 (45.3%) even presented a backward bending expansion path. We realize that subjects may be inattentive to the wealth monotonicity violation between two consecutive magnitudes which are not displayed in two neighboring decision forms, even though they can at any time go to any decision form easily by clicking a button. (See Figure A 3 for an example.) After removing those segments of which the two endpoints are not in neighboring decision forms, we still find 62 subjects (30.5%) presenting a segment of downward sloping expansion path.

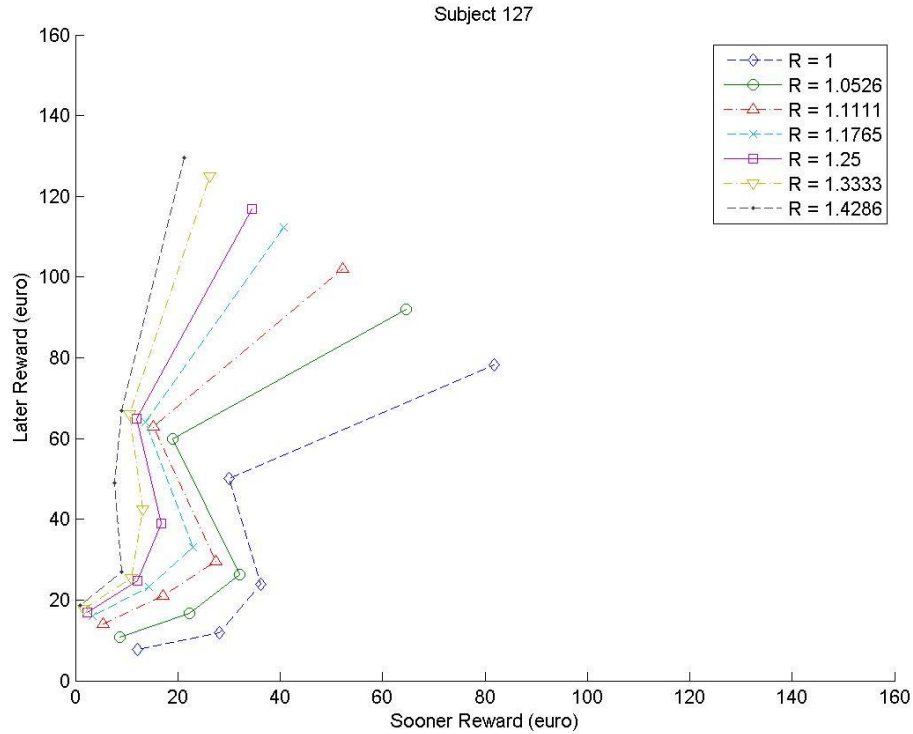


Figure A 3. An Example of Wealth Monotonicity Violation between Two Consecutive Magnitudes which were not Displayed in Two Neighboring Decision Forms

Notes: For this subject, the magnitudes were displayed in the following order: (€80, €40, €160, €60, €20). Notice that six violations of wealth monotonicity occurred between the magnitudes of €60 and €80, which were not displayed in neighboring

decision forms. One may argue that if they were displayed in neighboring decision forms, the subject might have paid attention to the monotonicity violations and tried to avoid them.

One may argue that the pattern may be explained by a tradeoff between benefits and costs of thinking: subjects make corner choices when they find the benefit of making a more precise choice cannot compensate for the thinking cost. However, this argument can only explain the downward sloping segments of which the endpoint for a lower magnitude is at a corner. Since 85 subjects (41.9%) chose an interior point for a lower magnitude while choosing a smaller sooner reward for a higher magnitude, we believe that thinking cost cannot fully explain the pattern.¹⁸

Lastly, we check how likely the pattern is driven by noise. Unfortunately, without imposing strong assumptions, such as homogeneity or a given standard error of the noise, it is very hard to formally test whether the pattern is purely due to noise. However, the following facts may cast some light on this issue: 39 subjects (19.2%) chose a segment with a difference of at least five tokens while still satisfying the two conditions mentioned above. And if we look at median choices of all subjects (Figure A 4) and median choices of all non-fully-integrating subjects (Figure A 5), i.e. those who do not put all the tokens onto the sooner date for a zero interest rate and all the tokens onto the later date for a positive interest rate, we still find a downward-sloping wealth expansion path.

¹⁸If we only count segments of which the two endpoints are in neighboring decision forms, then there are still 48 subjects (23.7%) presented a “interior-for-lower” segment of downward sloping expansion path.

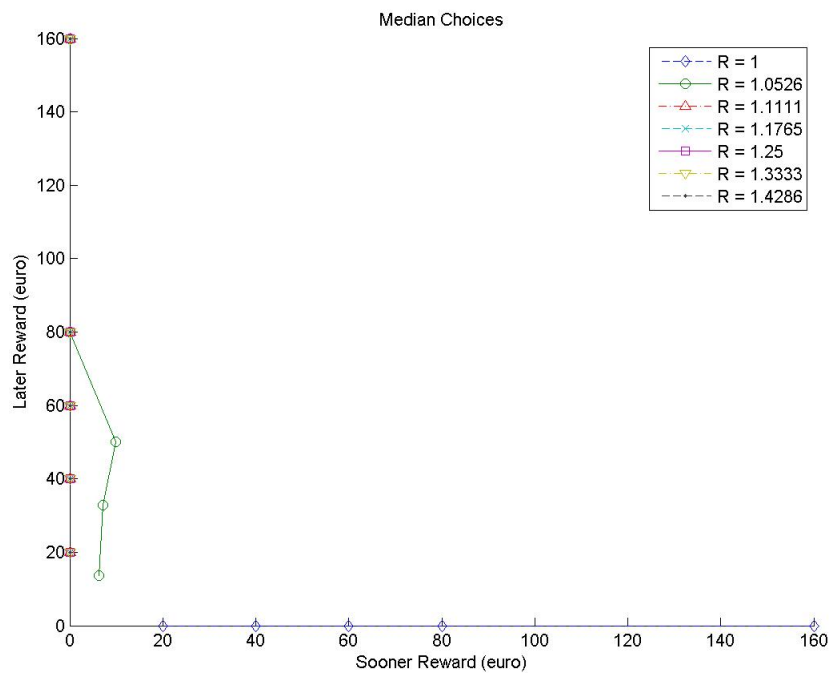


Figure A 4. Median choices of all subjects

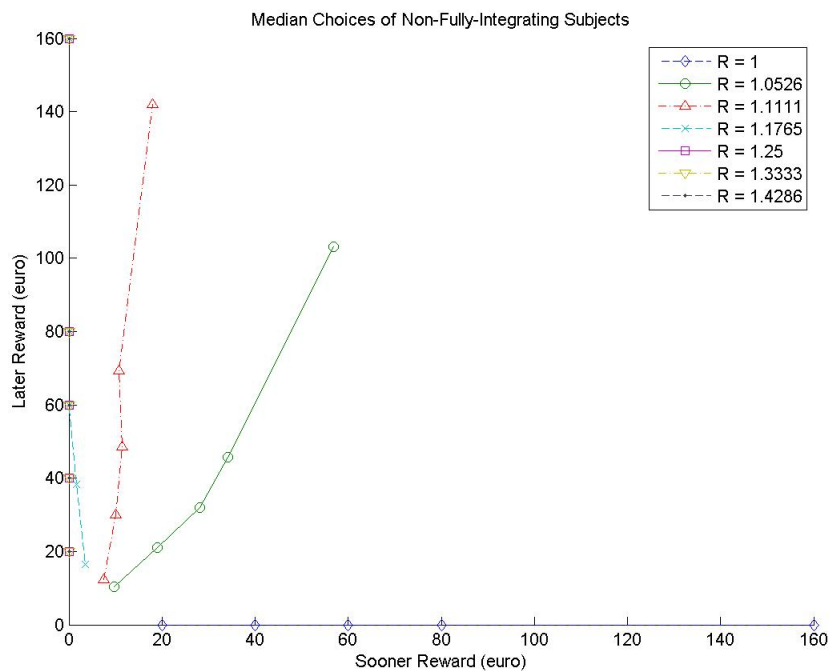


Figure A 5. Median choices of all non-fully-integrating subjects

Noor (2011) proposed a model of magnitude-dependent discounting. In such a model, the marginal discounted utility (which is the derivative of the product of the discount factor and the atemporal utility) can be increasing in the consumption since the newly added consumption not only contributes to the marginal utility directly, but also increases the contribution of the original consumption. While we have no idea if his model in general can rationalize the pattern, we realize that his parametric example, $\delta_m^{\frac{t}{\alpha}} \cdot u(m)$, is inconsistent with the pattern. The reason is that the marginal discounted utility in his parametric example is initially increasing and then decreasing, which can only predict a downward sloping jump from the sooner corner at a lower magnitude to an interior point/the later corner at a higher magnitude. His model cannot generate the interior-for-lower downward sloping segments mentioned above.

The pattern of the downward sloping wealth expansion path can be generated by the model with magnitude-dependent α . Since variation in α and variation in ω are perfectly substitutable, the pattern can also be generated by the mental zooming theory proposed by Holden (2014). For both models, mental accounting is a key characteristic. It seems that for many subjects different modes of decision making are triggered when the stakes are different. Nevertheless, it is still inconclusive whether the pattern is robust or is purely noise and hence further investigations are needed.

Appendix E. Decision Forms in Part II

Form 6: Decisions 36 - 42

September 2014	October 2014	November 2014	December 2014
1 2 3 4 5 6 7	1 2 3 4 5	1 2	1 2 3 4 5 6 7
8 9 10 11 12 13 14	6 7 8 9 10 11 12	3 4 5 6 7 8 9	8 9 10 11 12 13 14
15 16 17 18 19 20 21	13 14 15 16 17 18 19	10 11 12 13 14 15 16	15 16 17 18 19 20 21
22 23 24 25 26 27 28	20 21 22 23 24 25 26	17 18 19 20 21 22 23	22 23 24 25 26 27 28
29 30	27 28 29 30 31	24 25 26 27 28 29 30	29 30 31

You are required to allocate 400 tokens among the three dates for each decision:

	Payment A: 22 Sep TODAY	and	Payment B: 20 Oct 4 WEEKS from Today	and	Payment C: 17 Nov 8 WEEKS from Today	22 Sep	20 Oct	17 Nov
36	157 tokens at € 0.20 each on 22 Sep	and	43 tokens at € 0.20 each on 20 Oct	and	0 tokens at € 0.26 each on 17 Nov	€ 31.40	€ 8.60	€ 52.00
37	tokens at € 0.19 each on 22 Sep	and	tokens at € 0.20 each on 20 Oct	and	0 tokens at € 0.26 each on 17 Nov	€	€	€ 52.00
38	tokens at € 0.18 each on 22 Sep	and	tokens at € 0.20 each on 20 Oct	and	0 tokens at € 0.26 each on 17 Nov	€	€	€ 52.00
39	tokens at € 0.17 each on 22 Sep	and	tokens at € 0.20 each on 20 Oct	and	0 tokens at € 0.26 each on 17 Nov	€	€	€ 52.00
40	tokens at € 0.16 each on 22 Sep	and	tokens at € 0.20 each on 20 Oct	and	0 tokens at € 0.26 each on 17 Nov	€	€	€ 52.00
41	tokens at € 0.15 each on 22 Sep	and	tokens at € 0.20 each on 20 Oct	and	0 tokens at € 0.26 each on 17 Nov	€	€	€ 52.00
42	tokens at € 0.14 each on 22 Sep	and	tokens at € 0.20 each on 20 Oct	and	0 tokens at € 0.26 each on 17 Nov	€	€	€ 52.00

Submit Decisions

(a) The Present Group

Form 6: Decisions 36 - 42

September 2014	October 2014	November 2014	December 2014
1 2 3 4 5 6 7	1 2 3 4 5	1 2	1 2 3 4 5 6 7
8 9 10 11 12 13 14	6 7 8 9 10 11 12	3 4 5 6 7 8 9	8 9 10 11 12 13 14
15 16 17 18 19 20 21	13 14 15 16 17 18 19	10 11 12 13 14 15 16	15 16 17 18 19 20 21
22 23 24 25 26 27 28	20 21 22 23 24 25 26	17 18 19 20 21 22 23	22 23 24 25 26 27 28
29 30	27 28 29 30 31	24 25 26 27 28 29 30	29 30 31

You are required to allocate 400 tokens among the three dates for each decision:

	Payment A: 22 Sep TODAY	and	Payment B: 20 Oct 4 WEEKS from Today	and	Payment C: 17 Nov 8 WEEKS from Today	22 Sep	20 Oct	17 Nov
36	<input type="radio"/> 200 <input checked="" type="radio"/> 400 tokens at € 0.08 each on 22 Sep	and	<input type="text" value="0"/> tokens at € 0.20 each on 20 Oct	and	<input type="text" value="0"/> tokens at € 0.20 each on 17 Nov	€ 32.00	€ 0.00	€ 0.00
37	<input checked="" type="radio"/> 200 <input type="radio"/> 400 tokens at € 0.08 each on 22 Sep	and	<input type="text" value="150"/> tokens at € 0.19 each on 20 Oct	and	<input type="text" value="50"/> tokens at € 0.20 each on 17 Nov	€ 16.00	€ 28.50	€ 10.00
38	<input checked="" type="radio"/> 200 <input type="radio"/> 400 tokens at € 0.08 each on 22 Sep	and	<input type="text" value=""/> tokens at € 0.18 each on 20 Oct	and	<input type="text" value=""/> tokens at € 0.20 each on 17 Nov	€ 16.00	€	€
39	<input checked="" type="radio"/> 200 <input type="radio"/> 400 tokens at € 0.08 each on 22 Sep	and	<input type="text" value=""/> tokens at € 0.17 each on 20 Oct	and	<input type="text" value=""/> tokens at € 0.20 each on 17 Nov	€ 16.00	€	€
40	<input checked="" type="radio"/> 200 <input type="radio"/> 400 tokens at € 0.08 each on 22 Sep	and	<input type="text" value=""/> tokens at € 0.16 each on 20 Oct	and	<input type="text" value=""/> tokens at € 0.20 each on 17 Nov	€ 16.00	€	€
41	<input checked="" type="radio"/> 200 <input type="radio"/> 400 tokens at € 0.08 each on 22 Sep	and	<input type="text" value=""/> tokens at € 0.15 each on 20 Oct	and	<input type="text" value=""/> tokens at € 0.20 each on 17 Nov	€ 16.00	€	€
42	<input checked="" type="radio"/> 200 <input type="radio"/> 400 tokens at € 0.08 each on 22 Sep	and	<input type="text" value=""/> tokens at € 0.14 each on 20 Oct	and	<input type="text" value=""/> tokens at € 0.20 each on 17 Nov	€ 16.00	€	€

Submit Decisions

(b) The Delayed Group

Figure A 6. Interface of a typical decision form in Part II